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Essays on Labor and Public Economics

by

Krista Jean Ruffini

A dissertation submitted in partial satisfaction of the

requirements for the degree of

Doctor of Philosophy

in

Public Policy

in the

Graduate Division

of the

University of California, Berkeley

Committee in charge:

Professor Hilary Hoynes, Chair
Professor Jesse Rothstein
Associate Professor Christopher Walters
Assistant Professor Avi Feller

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Abstract

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This dissertation examines how three government programs targeted to disadvantaged populations – minimum wages, free school meals, and full-day schooling – affect labor market, education, and health outcomes. While each program examined here serves a different population, they all aim to improve the well-being of low-income groups. Moreover, each program is broad in its reach: more than 18 million workers would be affected by an increase in the minimum wage to $15 an hour (Congressional Budget Office, 2019), one-quarter of students attend a school offering schoolwide free meals, and an entire generation of students received 30 percent more instructional time in the full-day schooling reform we study. Yet the existing empirical work does not examine the full range of benefits each program offers. This dissertation aims to broaden our understanding how income assistance programs and educational interventions shape well-being over the lifecycle by examining factors that have received little attention to date.

The first chapter, *Worker Earnings, Service Quality, and Firm Profits: Evidence from Nursing Homes and Minimum Wage Reforms*, examines whether higher wages paid to low-income workers affects the quality of services they provide to consumers. To answer this question, I construct a novel dataset of administrative data on employment composition and patient health and safety for the near-universe of nursing homes spanning a twenty-five year period, and link this information with wage variation for direct care staff in driven by minimum wage reforms. My empirical framework builds upon existing approaches that isolate wage variation within narrow geographic areas, thereby accounting for local labor market conditions and demographic shifts.

I find that a ten percent increase in the minimum wage raises low-skilled nursing home workers' earnings one to two percent, reduces separations, and increases stable hires. These earnings gains and increases in firm-specific human capital translate into marked improvements in patient health and safety. A ten percent increase in the minimum wage would prevent at least 15,000 deaths, lower the number of inspection violations by one to two per-

cent, and reduce the cost of preventable care. Firms fully pass higher labor costs through to consumers by attracting patients with a greater ability to pay and increasing prices for these residents, resulting in no change in profitability. Considering costs elsewhere in the health system, savings from pressure ulcer treatment alone offsets up to half of the increased wage bill, and if the social value of increased longevity for nursing home residents is at least $21,000, well below existing estimates, higher wages in this sector are fully offset by improvements in care.

The second chapter, *Universal Access to Free School Meals and Student Achievement* and based on work forthcoming at the *Journal of Human Resources*, examines how providing schoolwide free meals affects school meal consumption and student academic performance. The school meals program is the largest nutritional assistance program for school-aged children and has undergone substantial changes in the past several years. Whereas program eligibility was historically determined by family income, recent reforms allow schools to offer free meals to all students. This paper evaluates the effect of the Community Eligibility Provision, the largest schoolwide free meals program, on academic performance. I leverage within- and across-state variation in the timing of CEP participation and find universal free meals increases breakfast and lunch participation by 38 and 12 percent, respectively. Math performance improves in districts with baseline low free meal eligibility, particularly for younger students and among racial/ethnic groups with low income-based participation rates. In contrast, there is no improvement in reading performance or significant changes among demographic groups with high participation rates under the traditional program.

The third chapter, *Long-Term Gains from Longer School Days*, based on joint work with Patricio Dominguez and revised and resubmitted to the *Journal of Human Resources*, examines another large-scale educational intervention – extending the time that children spent in school – on long-term economic well-being. Within the past 30 years, many emerging economies and middle-income countries have shifted their education systems away from a half-day model, where elementary and secondary students attended school for 4-5 hours a day, to a model where students attend school for 6-7 hours a day. While a large literature has focused on the contemporaneous effects of these programs on maternal employment and children's academic performance, little is known whether additional time in school confers lasting benefits as students enter the workforce.

We explore one of the first and largest such reforms. Between 1997 and 2010, Chile gradually increased the school day 30 percent for all elementary and secondary students in publicly-funded schools under the Jornada Escolar Completa (JEC) reform. Importantly, this reform required a large infrastructure investment and was rolled out over a fourteen-year period, providing variation in access to longer school days by both birth cohort and city of residence. We link administrative school enrollment data to household survey information on labor market outcomes based on survey respondents' age and city of birth and compare economic well-being between students with different access to JEC who were born in the same geographic region. We find important benefits to additional time in school: full-day schooling increases educational attainment, delays childbearing, and increases earnings in young adulthood. The nature of these benefits is consistent with more time in school facilitating human capital accumulation, and our results show that large-scale investments in public education can generate long-term improvements in economic well-being.

Combined, these works quantify important benefits of educational and workplace investments that have received little attention in the previous literature. Each chapter also explores how benefits vary across socioeconomic and demographic groups, highlighting that the effects of policy reforms depend on individuals' resources and interactions with other existing programs.

To my grandparents

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Chapter 1

Worker Earnings, Service Quality, and Firm Profits: Evidence from Nursing Homes and Minimum Wage Reforms

1.1 Introduction

While the quality of goods and services affects consumer well-being, in many settings employers and customers cannot discern quality at the time of production or purchase. In these situations, front-line workers may have weak incentives to provide high-quality services and products, and paying employees higher wages can lead to improved output quality through standard efficiency wage arguments (Shapiro and Stiglitz, 1984; Akerlof, 1982; Lazear and Moore, 1984). Beyond standard principal-agent considerations, low-wage industries commonly experience high employee turnover. To the extent that higher wages reduce the arrival rate of better-paying jobs and increase worker tenure, increases in job-specific expertise can improve production efficiency.

Although the theoretical motivations for offering efficiency wages and building firm-specific human capital are well developed, the existing empirical work on these topics is largely limited to workers in production industries where quality is often readily observable. In contrast, there is little evidence whether higher worker compensation affects consumer outcomes in low-wage service industries where employee effort is particularly difficult to monitor and quality is subjective or not easily quantified. Moreover, whether wage increases induced by government *policy* can serve an efficiency wage function remains an unanswered question.

This chapter broadens our understanding of how low-skilled employees' compensation translates into consumer well-being by examining the relationship between direct-care workers' wages and patient health and safety in long-term residential care settings. I measure consumer outcomes using objective measures of patient health and safety for the near-universe of nursing homes, and leverage 25 years of wage increases for healthcare support staff driven by minimum wage reforms. This source of wage variation – statutory minimum wages – differs in important aspects from wage increases set by employers as part of profit maximization decisions. While both minimum wages and voluntary wage increases could improve service quality by attracting more productive workers and incentivizing greater worker effort, mandated wage increases may also prompt firms to reduce the number of staff, leading to worsened quality of care. If the number of workers and their effort affect output, the direction and magnitude of the relationship between higher minimum wages and service quality is *a priori* uncertain and an empirical question.

In order to test whether legislated wage increases can perform an efficiency wage function, my empirical framework adapts the contiguous county-pair border design, pioneered by Card and Krueger (1994) and generalized by Dube et al. (2010) and Dube et al. (2016). I build upon this approach by including city- and establishment-level reforms, in addition to state and county changes, and measuring outcomes at the establishment level. The rich temporal and spatial variation in minimum wages allows me to flexibly account for demographic and economic changes at a very local level and compare changes in patient well-being within the same facility due to exogenous changes in labor costs.

I first establish minimum wages increase earnings among workers in a sector that has received relatively little attention in the US literature – low-skilled healthcare occupations. In particular, a ten percent increase in the minimum wage increases average earnings of nursing assistants and other low-skilled nursing home staff by approximately 1.2 to 2.0 percent. Although this is an unexplored industry, I find the earnings response among affected workers is comparable to that found in other low-wage industries (Dube et al., 2010, 2016; Reich et al., 2017; Jardim et al., 2017; Cengiz et al., 2019). Also consistent with previous work, employment does not significantly fall for nursing assistants and vocational nurses, however, there is a slight decrease in the amount of time RNs spend with patients. While employment levels of low-skilled staff do not fall, higher minimum wages also reduce the worker separation rate and increase the share of new hires who remain with their employer for at least three months, consistent with work documenting that higher minimum wages increase tenure (Dube et al., 2016; Jardim et al., 2018; Portugal and Cardoso, 2006; Brochu and Green, 2013).

Second, I provide some of the first empirical work on how higher wages paid to workers affect consumer well-being and document that higher minimum wages improve patient safety and health. A ten percent increase in the minimum wage reduces the number of health inspection violations by one to two percent (0.1 violation for the typical facility each inspection) and the fraction of residents with moderate-to-severe pressure ulcers by approximately 1.7 percent (0.14 percentage points). Beyond these intermediate outcomes, higher wages also yield substantial reductions in age-adjusted mortality, with a ten percent increase in the minimum wage leading to approximately 15,000-16,000 fewer deaths a year (a 3.1 to 3.3 percent reduction). Event study analyses indicate that health patterns do not systematically diverge prior to minimum wage increases relative to outcomes in adjacent counties, providing strong support for the basic differences-in-differences research design, and that reductions in the most costly health outcomes – pressure ulcers and mortality – persist after the initial wage shock.

Third, to fully understand how higher wages affect the industry, I turn attention to the firm and examine whether employers are able to offset higher labor costs. I find that the mechanical increase in a firm's costs from the baseline labor share is nearly identical to reported cost changes. Rather than substituting towards higher-skilled labor or other factors of production, firms fully pass higher costs through to consumers in the form of higher prices. This price channel takes two forms: about 75 percent of the increase in per-resident revenue is due to firms charging private payors more – there is no change in the average Medicaid or Medicare per diem. The remaining 25 percent is due to firms serving more high-revenue patients – that is, serving more private payors and fewer Medicaid recipients.

My findings are consistent with minimum wages improving patient health through increasing firm-specific human capital and improving worker performance. Importantly, the underlying motivation for efficiency wages – namely that higher compensation improves worker productivity when effort is *imperfectly observable* to employers and consumers – makes disentangling potential mechanisms particularly difficult in this setting. In the absence of a direct measure of worker productivity, I conduct several analyses to rule out competing explanations. First, higher labor costs may incentivize providers to target resources (residential beds) to patients who provide greater revenue conditional on care needs. Although I find nursing home residents become more positively selected following minimum wage increases, changes in observable patient characteristics can account for at most 12 percent the observed health improvements and mortality reductions. Second, I do not find minimum wage increases lead to higher rates of firm closure or ownership changes, suggesting improved patient outcomes are not driven by low-performing firms exiting the market. Third, higher wages could improve quality by altering incentives of current workers, attracting better workers to low-wage positions, or leading firms to substitute towards higher-skilled staff. I find suggestive evidence that higher wages lead to increased firm-specific human capital, measured by lower separations and longer periods of employment for new hires, but no significant changes in worker demographic characteristics or occupational composition. Taken together, these patterns are consistent with higher minimum wages inducing greater effort among current workers and improving production efficiency through greater retention.

These findings provide new empirical evidence on how higher wage mandates can improve service quality in low-wage settings. Although long-term care is a single industry characterized by relatively inelastic consumer demand, this sector is an interesting and important setting to explore the relationship between worker economic security and consumer outcomes for several reasons. First, long-term and elder care services is a large and rapidly growing sector of the economy, accounting for about 10 percent of Medicaid and Medicare expenditures. More than half of individuals reaching age 65 will require long-term care at some point in their lives, much of which is provided in residential settings (Favreault and Dey, 2015). Patients have imperfect information about the quality of care at the time of admission, as health conditions develop over time and require expertise to diagnose. The traditional fee-for-service reimbursement model, combined with a large role of government financing through Medicaid and Medicare, results in relatively inelastic consumer demand that reduces incentives for providers to offer cost-efficient treatments. As the population ages, demand for long-term care will increase, placing greater pressure on government finances and increasing demand for health services workers.

Second, medical experts, policymakers, families, and patients have expressed concerns about the quality of long-term care for at least sixty years (Castle and Ferguson, 2010; Institute of Medicine, 1986). In an effort to monitor service provision, the federal government has implemented a series of inspection and reporting requirements. I build a novel dataset from this information that includes administrative measures of staffing, health inspection violations, and patient health for the near-universe of nursing homes spanning the 1991 through 2017 period.

This chpater contributes to three distinct literatures in economics and public health. First, an extensive literature examines the labor market effects of higher minimum wages. The previous work concludes that a ten percent increase in the statutory minimum wage increases earnings among affected groups by approximately two percent (CBO, 2019, Wascher and Neumark (2007), and Schmitt (2013) provide comprehensive overviews of the recent literature).[1] Employment effects are more contentious, but center around zero (Belman and Wolfson, 2014; Doucouliagos and Stanley, 2009), with studies employing substate geographic controls tending to find null results (Card and Krueger, 1994, 2000; Dube et al., 2010, 2016; Allegretto et al., 2011, 2017; Totty, 2017), and those using state-by-year fixed effects models tending to find significant disemployment effects (Neumark and Wascher, 1992; Deere et al., 1995; Neumark et al., 2014).[2] This existing research, however, is largely limited to two sectors, the retail and food services industries, and is sensitive to the time period examined (Cengiz et al., 2019). My results indicate that relatively recent minimum wage reforms increase earnings among low-wage workers by a comparable amount, but in a different setting (low-wage healthcare support), without significantly reducing employment.

Second, a smaller literature examines the effects of minimum wages on consumer well-being, and documents that higher minimum wages increase consumer prices (Draca et al., 2011; Harasztosi and Lindner, 2015; Aaronson et al., 2008; Allegretto and Reich, 2018). Other customer outcomes, however, are relatively unexplored. This chapter provides some of the first empirical evidence of how higher minimum wages can affect consumer well-being on non-financial dimensions in a setting where employers and consumers are unable to perfectly monitor worker effort. One notable exception is Giupponi and Machin (2018), who examine the effect of a single, national minimum wage reform in UK nursing homes and find higher

[1] Recent work also documents that higher minimum wages stimulate income mobility and lead to long-term earnings growth for affected workers (Rinz and Voorheis, 2018), consistent with an earlier literature showing higher minimum wages reduce income inequality at the bottom of the income distribution (DiNardo et al., 1996; Lee, 1999; Lemieux, 2008; Autor et al., 2016).

[2] Employment responses may differ across worker types. While some work finds disemployment effects concentrated among less-experienced workers, or in poor economic conditions (Jardim et al., 2018; Meer and West, 2016; Sabia et al., 2012; Addison et al., 2013; Clemens and Wither, 2019; Gittings and Schmutte, 2016), other work finds higher minimum wages decrease overall employment, but increase the share of teenage workers in relatively high-wage labor markets (Giuliano, 2013; Lang and Kahn, 1998).

minimum wages increase the number of inspection violations. One important difference between these settings is that British nursing homes have limited ability to increase prices, whereas I document that higher labor costs are fully passed through to consumers in the form of higher prices and a shift from relatively low-revenue Medicaid recipients to high-revenue private payors. More generally, several papers examining whether pay schedules for public-sector employees affect service quality find that higher wage *ceilings* set by regulation improve the service quality, measured by hospital deaths (Propper and Van Reenen, 2010) and student test scores (Britton and Propper, 2016). This chapter complements the existing work by showing that higher wage *floors* targeted towards relatively low-skilled staff likewise can improve consumer outcomes.

Third, this chapter relates to a literature examining how personnel policies in the long-term care sector affect patient outcomes. The previous work in this area finds that increased staffing due to changes in regulations and macroeconomic conditions reduce mortality and lower the number of inspection violations (Chen and Grabowski, 2015; Matsudaira, 2014a; Antwi and Bowblis, 2018; Stevens et al., 2015). However, the relationship between staffing levels and other measures of patient health is more mixed (Matsudaira, 2014a; Chen and Grabowski, 2015; Bowblis, 2011), and may be due to changes in the skill mix of nursing staff if facilities respond to minimum staffing requirements by substituting towards less-skilled direct care staff (Matsudaira, 2014b; Chen and Grabowski, 2015). This chapter complements the existing literature by examining the effect of *price* regulations on patient outcomes and changes in the skill mix of nursing home workers. I find that higher wages improve patient well-being by at least as much as comparably-priced staffing requirements, suggesting that modest wage increases may be more cost-effective than increasing employment levels alone. In addition, my findings suggest higher wages have benefits net of existing quantity regulations and macroeconomic conditions. In particular, all of the main results account for business cycle fluctuations and state income assistance programs, and I do not observe differential responses among facilities in states with a regulations requiring a minimum number of direct care workers.

The remainder of this chapter proceeds as follows. Section 1.2 describes the nursing home industry. Section 1.3 presents a conceptual framework outlining how legislated wage increases may affect worker, consumer, and employer well-being, measured by prices, costs, and quality. Section 1.4 describes the cross-county border pair empirical approach. Section 1.5 presents results, and Section 1.6 concludes.

1.2 Institutional setting

There are approximately 15,600 nursing homes (also called nursing facilities) in the United States that provide 24-hour health, personal care, supportive, and rehabilitative services to about 1.4 million residents.[3] Nursing home residents require assistance with activities of

[3] Approximately 92 percent of certified nursing home facilities are dually certified as skilled nursing facilities (SNF) (HHS, 2015). SNFs provide services that can only be provided under the supervision of a

daily living (ADL), such as eating, bathing, dressing, mobility, and toileting. Most nursing home residents are elderly; more than 40 percent are 85 or older (HHS, 2015). Demand tends to be high relative to supply, and most nursing homes operate near capacity.[4]

Nursing home employment

Nursing homes are labor-intensive enterprises and a large employer of low-wage workers. These facilities employ about 1.6 million workers, approximately 40 percent of whom work in healthcare support roles as nursing assistants.

The duties of nursing staff, particularly nursing assistants, have potentially large consequences for patient health and longevity. Nursing assistants record vital signs, monitor health outcomes, report health conditions to certified nurses, and provide medical and personal care to residents. This care can take the form of "emotional labor," or conversing with patients and building worker-client relationships (Hochschild, 2012), as well as administering medications and treatments or helping residents with transportation, feeding, bathing, and mobility (ONET, 2018). Most patients rely on assistance for daily activities: approximately 60 percent require help eating, and more than 90 percent require help walking, dressing, and bathing (CDC, 2014).

The typical healthcare support worker in the long-term care industry receives about $13 an hour, comparable to wages in other low-pay sectors (BLS, 2019).[5] Turnover is also high in this sector: 62 to 86 percent of nursing assistants change employers each year (Berridge et al., 2018), with most of these transitions occurring among nursing homes. While most nursing assistants have worked as nursing assistants for at least six years, most had worked for at least two employers in this period (CDC, 2004).

To situate nursing homes in the broader minimum wage literature, Table 1.1 compares wages and demographic characteristics of nursing staff with the largest low-wage industries – food service and retail workers. Table 1.1 shows that nursing assistant wages are slightly higher than restaurant workers, but comparable to retail workers and lower than the private sector average (see also Appendix Figure A.1).[6] In contrast, licensed nursing staff receive wages higher than typical private sector workers. These statistics imply that while many

registered nurse or doctor. Medicare provides full coverage for the first 20 days of SNF care following hospital discharge, and co-insurance for the subsequent 80 days. Medicare does not coves nursing home care, but dually certified facilities can receive both Medicaid and Medicare reimbursement.

[4]In 2016, the median occupancy rate was 85 percent, and 15 percent of facilities had an occupancy rate greater than 95 percent.

[5]This estimate is based on dividing annual earnings by 2,080 (40 hours a week times 52 weeks a year). Overtime is common among nursing assistants: approximately 20 percent report being subject to mandatory overtime, and about half report being able to work overtime hours (CDC, 2004). As overtime pay is most frequently paid at 1.5 times the normal hourly wage, this figure likely overstates hourly wages.

[6]Following Dube et al. (2019), the fraction of workers affected by a 10 percent minimum wage increase is estimated as the fraction of workers earnings below 110 percent of the current minimum ("directly affected") and those earnings between 110 percent of the current minimum and 115 percent of the new minimum ("indirectly affected"). Consistent with this estimate, Cengiz et al. (2019) document that workers earning $3 above the minimum wage may be affected by spillovers, and indirectly affected workers account for 40

nursing assistants are likely affected by minimum wage reforms, LPNs and RNs are not. Examining wage responses among these higher-skilled occupations provide a placebo test, as null earnings effects for licensed nurses suggests that the empirical design is not simply capturing economy-wide wage increases.

Quality of care and patient health outcomes

Nursing homes are subject to extensive federal reporting and inspection requirements. Federal care standards date to 1961, and were strengthened under the 1987 Nursing Home Reform Act (NHRA) after the Institute of Medicine concluded that existing regulations were "shockingly deficient" (Castle and Ferguson, 2010; Institute of Medicine, 1986). The NHRA required annual independent health inspections; greater nursing credentialing; minimum RN staffing; and routine, comprehensive patient assessments. Nursing homes must submit this information to the federal government in order to qualify for Medicaid and Medicare reimbursement. I leverage this information in order to provide a comprehensive analysis of consumer well-being.

Firm payment sources and revenue

Nursing homes operate in a regulated market with limited scope for free entry and relatively inelastic consumer demand. On the supply side, nursing home services are largely fixed at both the facility and aggregate level: most states have a certificate-of-need (CON) law, which restricts construction and limits the number of beds each facility can provide (NCSL, 2019; HHS, 2015).

On the demand side, demand for nursing home care is relatively price inelastic, as there are few close substitutes for intensive nursing services and most residents do not incur the costs of the services they receive. Only about one-quarter of residents pay for care from their own funds or private insurance, the remainder are covered by either Medicare (14 percent) or Medicaid (62 percent) (Kaiser Family Foundation, 2016). Both Medicare and Medicaid rates are set by expected patient costs. Medicare reimbursement rates depend on each resident's service needs with a local cost-of-living adjustment (AARP, 2018; CMS, 2019; Federal Registry, 2018).[7] Medicaid rates and payment structure vary by state, with an average daily rate of $195, typically received regardless of individual care needs. Private rates are set based on actual costs incurred at market rates, averaging $263 a day (a 30 percent premium over

percent of the increased labor costs.

[7]Medicare reimbursement is based on the expected cost of care, determined by an intake assessment. Residents who are expected to require help with a large number of daily activities, or who require extensive rehabilitation or therapeautic services are assigned a relatively high reimbursement rate (Resource Utilization Group, or RUG group). In 2019, the base reimbursement rate ranged from $209 (for those who require assistance with 4-5 ADLs and no rehabilitative services or special care) to $832 (for residents who require "ultra-high and extensive" rehabilitative services and assistance with 16-18 ADLs) a day. These rates are then adjusted by a local cost index.

Medicaid rates, Appendix Table A.1). While private payors are expected to respond to both price and quality, those covered by public insurance are likely less responsive to changes in either prices or quality (Gertler, 1989). Given a fixed supply of beds, these patterns suggest firms may to respond higher labor costs by either altering employee composition, increasing prices, or adjusting the resident mix towards residents who generate greater revenue, net of service costs. Section 1.3 outlines facilities' objectives and incentives more formally, and Section 1.5 empirically examines changes in resident composition.

1.3 Conceptual framework

Higher wages may affect the cost, quality, and quantity of nursing home services. This section presents a stylized framework illustrating how workers, firms, and clients respond to mandated wage increases, and shows that higher minimum wages could wither improve or worsen the quality of nursing home care. To fix ideas, I simplify, and adapt to the long-term care setting, the analysis in Feldstein (1977). While the discussion here is conceptual, Appendix A.3 presents a more thorough welfare analysis and illustrates settings in which accounting for product quality is a first-order consideration.

In the basic model, there are three types of agents: nursing home employees, firms, and consumers. Workers supply labor to maximize utility; firms choose staffing levels and resident composition to maximize profits; and potential consumers choose the quantity of nursing home care they receive to maximize utility.

Worker utility maximization Workers have utility $U(w, e) = u(w) - b(e)$, increasing and concave in consumption w, and decreasing and convex in effort e. The effort function, $b(.)$ depends on individual characteristics, such as a comparative advantage in caregiving, as well as her career history and firm-specific human capital.[8]

Employers are unable to perfectly monitor worker effort, but observe a noisy signal of effort $\widehat{e} = e + \mu$ where $\mathbb{E}(\mu) = 0$. Workers without employment have $\overline{w}$ available for consumption through either unemployment insurance or income assistance benefits. The unemployed do not incur effort costs, and receive utility $U(\overline{w})$. Each period, those without employment can begin work and those with employment may enter unemployment, with the probability of entering unemployment decreasing in observed effort, $\widehat{e}$. Taking wages and separation and hiring rates as given, workers choose effort by equating the expected utility while employed to what they would be expected to obtain in unemployment. Appendix A.2 derives these conditions more formally, but at an intuitive level, higher wages induce greater effort by

[8]Increases in occupation- or firm-specific human capital shift the effort function, resulting in the observationally-similar pattern as a worker putting forth greater effort holding b constant. Higher wages could affect both the b and the e terms; while I am unable to fully disentangle between these mechanisms, I document some of the documented improvements can be attributed to increases in firm-specific human capital.

increasing the opportunity cost of unemployment, as in Shapiro and Stiglitz (1984).[9] With diminishing marginal value of consumption, effort rises less than in proportion to wage increases ($e^{'}(w) > 0$ and $e^{''}(w) < 0$, shown in panel (a) of Figure 1.1). In more dynamic settings, effort may change as a worker gains expertise in her role or familiarity with her workplace and colleagues, shifting the $b(.)$ function so that providing a given effort is less "costly" to the worker (panel (b) of Figure 1.1).

Firm profit maximization Firms have some market power in the product market and produce consumer health of quality Q_N with labor, L, and non-labor, K.[10] Importantly, Q_N can depend on both the quantity of inputs, L and K, and workers' effort levels. Greater worker effort e leads to higher quality of services provided. As worker effort is monotonically increasing in wages, the healthcare production function can be written as $Q_N = Q_N(L, w, K)$, where $\frac{\partial Q_N}{\partial L} > 0$, $\frac{\partial Q_N}{\partial K} > 0$, $\frac{\partial Q_N}{\partial e}\frac{\partial e}{\partial w} > 0$. All else equal, higher wages improve quality.

Firms incur average costs per resident day of $C = wL + rK$, and receive average per-resident revenue P. Firms' operating losses are then $D = P - C$, where $D < 0$ denotes positive profits.

The quantity of health produced is constrained by the firm's supply of nursing home beds, X, which is governed by state regulation, such as CON laws. Subject to the resource constraint X, firms maximize profit by choosing the cost-minimizing combination of inputs and quality, assuming workers will choose the optimal effort level for a given wage.[11] The profit-maximizing wage level is set such that the marginal value of additional effort equals the additional productivity of an additional factor (Figure 1.1) (Solow, 1979):

$$max_{L,w,K}\pi = PX(Q, P, I, Z) - C \text{ subject to } P - C \geq D \tag{1.1}$$

Consumer demand Prospective nursing home clients have demand for nursing home beds given by $X = X(P_c, Q, I, Z)$ where P_c is the price nursing homes charge residents, which can differ than the amount actually paid after insurance; Q is perceived quality, which is (potentially imperfectly) correlated with actual quality Q_N; I is insurance coverage that offsets market prices; and Z is a vector of individual characteristics. Demand is decreasing in net price (therefore increasing in insurance coverage) and increasing in quality $\frac{\partial X}{\partial P} < 0$,$\frac{\partial X}{\partial I} > 0$, and $\frac{\partial X}{\partial Q} > 0$.

As discussed in Section 1.2, nursing homes serve three types of patients – those who pay out of pocket, and those with insurance coverage through Medicaid or Medicare. For simplification, this section pools Medicaid and Medicare recipients as the weighted average of these two groups.[12] At one extreme, private payors, denoted by subscript p have no

[9]As minimum wages increase wages for all employers, employee effort is unlikely to respond via a "gift" mechanism as in other efficiency wage models (e.g.: Akerlof (1982)).

[10]Without loss of generalization, labor can be of multiple types L_i.

[11]If the labor market is imperfectly competitive, the wage level is also a choice variable. If the labor market is perfectly competitive, all firms take wages as given, and equilibrium the (sole) prevailing wage will be that which satisfies $e^{'}(w)^{-1} = w$ (Solow, 1979).

[12]The implications are identical, but notationally more cumbersome, when disaggregating these groups.

insurance coverage, $I_p = 0$ and pay the rates nursing homes charge $P_p = P_c$. When prices increase, these consumers demand fewer services. Holding prices constant, increased quality increases demand.

At the other extreme, Medicaid and Medicare beneficiaries, denoted by subscript g, have full insurance coverage $P_c - I_g = P_g = 0$. These clients are unresponsive to price changes and fill any remaining beds after firms meet private demand. Medicaid patients are less attractive from the firm's perspective Aa Medicaid reimburses facilities at a rate lower than the charged amount, $P_g < P_c$ (Gertler, 1989). P, the relevant revenue for setting prices and quality from the firm's perspective, is the weighted average of the resident groups.

Market equilibrium, no minimum wages Figure 1.2 displays equilibrium prices, quality, and costs in the absence of minimum wages. In equilibrium, demand for nursing home beds equals supply. Holding price constant, higher quality increases demand for nursing home services; therefore, when quality improves, firms will raise prices in order to satisfy the bed constraint. On the other hand, when quality worsens, average per-resident revenue decreases. Line DS_0 in Figure 1.2, Quadrant I denotes all market clearing price and quality combinations.

Higher quality care requires additional inputs and therefore greater costs ($\frac{\partial Q}{\partial L} > 0$, $\frac{\partial Q}{\partial K} > 0$, $\frac{\partial Q}{\partial e}\frac{\partial e}{\partial w} > 0$, and $r, w > 0$). With diminishing marginal factor products, each additional worker or equipment provides a smaller quality improvement than the previous input, resulting in a strictly increasing, concave cost-quality relationship, depicted as line CQ_0 in Figure 1.2 Quadrant III.

Finally, for a given operating loss D, Figure 1.2 Quadrant IV graphs the budget constraint.

Connecting the set of cost-quality combinations that are feasible with the available technology (Quadrant III) and the firm's the budget constraint (Quadrant IV) provides line FC_0 in Quadrant I of Figure 1.2. Market equilibrium is the price-quality combination that is both technologically feasible and equates supply and demand – the intersection of DS_0 andFC_0. In the absence of minimum wages, quality, prices, and costs given by $Q_N = Q_0$, $P = P_0$, and $C = C_0$, respectively.

Market equilibrium, minimum wages A minimum wage increase changes prices and quality by altering firms' costs and the production technology. If firms select inputs (and therefore quality) and prices to maximize profits before a minimum wage increase, higher minimum wages shift the production function away from the profit-maximizing (cost-minimizing) combination (from (e_0, w_0) to (e_1, w_1) in Figure 1.1). Accordingly, costs increase for any quality level, shown in panel (a) of Figure 1.3. Given that higher wages increase the "effective" number of workers by making each employee more productive, employment among low-skilled labor, and potentially other factors, will decrease. In the new equilibrium, quality falls to Q_1 and prices decrease to P_1 in order to satisfy the bed constraint.

Alternatively, minimum wages may change firms' budget constraint such that profits fall (panel (b) of Figure 1.3). Such a scenario can arise in settings where firms face an upwardsloping labor supply curve (due to efficiency wages, monitoring costs, search frictions, or bargaining power, see Manning (2003), Card and Krueger (2015), and Rebitzer and Taylor (1995) for examples). In this case, any higher costs are borne by the firm, and both prices and quality may increase.

Another possibility is that quality may improve without reducing firm profits. This scenario arises with increasing marginal product of worker effort (a convex relationship between costs and quality in Quadrant III), but in more realistic settings, such a pattern could appear if higher wages change production technology – for example, by increasing worker retention or reducing other costs associated with low effort, such as human resource staff needing to monitor workers' effort, job training services, or fees paid for violations. Another possibility is that facilities may have imperfect information about their production function, in which case legislated wage increases can shift firms towards the production frontier (similar to mechanisms in Flinn (2006)). This setting is the converse of that in Figure 1.3, panel (a). In equilibrium, minimum wages increase both prices and quality, without necessarily reducing profits.

This framework yields three empirically testable predictions:

1. **If staffing levels fall and average tenure does not increase, quality is expected to worsen**; conversely, if staffing levels and tenure weakly increases, quality is expected to improve. If staffing levels fall and tenure increases, the effect on quality is ambiguous.

2. **Improved quality is expected to increase consumer prices**. Higher quality may also lower firm profits.

3. **Quality improvements are expected to shift services away from Medicaid recipients towards private payors** since Medicaid residents generate less revenue than those paying out of pocket.

This simplified setting also illustrates that the costs and benefits of higher wages not evenly distributed. From a social welfare perspective, the desirability of minimum wages depends on the social welfare weights attached to low-wage workers, firm owners, taxpayers, and customers; changes in access to health services; and the relationships among worker earnings, consumer health, and resident prices. Appendix A.3 describes these tradeoffs in greater detail.

1.4 Empirical framework

In order to empirically examine how minimum wages affect the quality of nursing home care, I extend the cross-border differences-in-differences approach pioneered by Card and Krueger (1994) and generalized by Dube et al. (2010) and Dube et al. (2016) by comparing changes in patient outcomes within a nursing home to changes among facilities in neighboring counties following an increase in the statutory minimum wage.[13]

The prevailing minimum wage is defined as the statutory minimum wage (the maximum of the federal, state, county, or city minimum) applicable to a facility the date the outcome of interest was measured, adjusted for inflation using the CPI-U-RS.[14] Figure 1.4 shows that minimum wage increases are frequent events, and each year, a large fraction of nursing home residents in the county pair sample live in a jurisdiction with a minimum wage reform. A large share of these changes is due to federal legislation (1996-1997 and 2007-2009), while policy action is driven by state – and more recently, local – legislation in the intervening years.

Importantly, minimum wages vary within narrow geographic areas, and this spatial variation has increased over time. Figure 1.5 shows the difference in log minimum wages faced by nursing homes in adjacent counties from 2002 through 2017. Darker shades correspond to larger cross-border gaps. In the late 1990s and early 2000s, minimum wage variation was concentrated in the Middle Atlantic, New England, and Western states; by the end of the period, approximately one-third of nursing home patients lived in a jurisdiction where an adjacent county had a different minimum wage, including residents in the midwest and some southern states. In total, the main county pairs sample consists of approximately 7,700 facilities in 1,136 counties. On average, a county experienced about seven minimum wage reforms over the 1990 through 2017 period.[15]

The basic set-up is a generalized differences-in-differences model, comparing changes within a facility relative to changes in neighboring facilities following a minimum wage increase. The mortality and QWI-based employment measures are aggregated at the county level (c), other measures are estimated at the facility level (f). For each outcome $y_{c(f)pt}$ in county c or facility f, in county border pair p at time t, I estimate:

$$y_{c(f)pt} = \beta log(MW)_{c(f)pt} + X'_{c(f)pt}\phi + \gamma_{c(f)} + \gamma_{pt} + \varepsilon_{c(f)st} \tag{1.2}$$

where $log(MW)_{c(f)pt}$ is the prevailing real minimum wage in county c or facility f.[16] $X'_{c(f)pt}$ is a vector of county and facility controls, including the overall county unemployment rate;

[13]This "county pairs" sample includes establishments in counties straddling a state border (such as Illinois and Indiana), those within a state (such as Cook County (Chicago) and De Kalb county), and those in cities bordering a county with a different minimum (such as facilities in the city of Berkeley, California and adjacent Contra Costa County, California).

[14]In addition to the geographic variation in minimum wages, facilities receiving federal contracts and subcontracts are subject to a \$10.10 minimum wage (in 2015 dollars) for years 2015 and later under Executive Order 13658. I identify federal contractors by matching nursing home addresses to contractor information from the Federal Procurement - Next Generation Data System (USASpending.gov, 2018). This policy affects approximately four percent of nursing facilities. Results are nearly identical when excluding federal contractors.

[15]Appendix Table A.2 presents descriptive statistics for this sample compared to facilities in all counties, and indicates the county pair sample is similar to the full universe of nursing homes in staffing levels per resident, resident demographic characteristics, number and severity of inspection violations, and patient outcomes.

[16]For counties where a subset of jurisdictions have higher local minimum wages, I assign the maximum

state income assistance and tax policy controls; race and gender characteristics of nursing home residents; payment sources of nursing home residents; and the population age structure. While average facility characteristics are included primarily for precision, labor market controls account for factors that may affect policymakers' decisions to change minimum wages, elderly health, or nursing home staffing. $\gamma_{c(f)}$ is a county (facility) fixed effect accounting for time invariant county (facility) characteristics, and γ_{pt} is a county-pair-by-time fixed effect that accounts for local features that evolve over time, including labor market conditions and changes in the share of residents requiring long-term care.[17]

β provides the causal effect of a 100 log point increase in the minimum wage under the assumption that within a county pair and conditional on county unemployment, resident demographic characteristics, and other state policies, minimum wage increases are uncorrelated with changes in potential health of nursing home residents. Importantly, facilities on either side of the policy discontinuity operate in the same local labor market and are therefore likely to experience similar business cycle fluctuations and serve similar potential resident populations.

Table 1.2 examines whether facilities facing higher minimum wages within the county pair systematically differ from competing firms across the border by regressing a series county characteristics on an indicator for whether a facility is located in the highest minimum wage jurisdiction within a county-pair year. This table shows the average within-pair minimum wage difference is approximately 57 cents (in 2017 dollars), but county unemployment, population, and facility size do not significantly differ between low and high- minimum wage counties. While facility characteristics suggest nursing homes subject to higher minimum wages have a smaller share of female residents and a slightly younger population, the point estimates in column (2) are small relative to the average values in column (1). In light of these small differences in resident demographics, my preferred specifications control for all time-varying resident characteristics, as well as level differences in all characteristics. Furthermore, in robustness checks, I demonstrate that changes in demographic characteristics coinciding with minimum wage reforms can only account for a small fraction of the observed improvements in patient health and safety.

Previous work has documented that minimum wage reforms are geographically concentrated and correlated with regional business cycle fluctuations. Even with the inclusion of state and year fixed effects, models that do not account for local economic conditions are prone to omitted variable bias concerns (Allegretto et al., 2017). The county pair de-

minimum wage prevailing within that county. For example, in January 2017, the minimum wage in Alameda County was \$10.50, and in Berkeley was \$12.53. My main specifications assign the Berkeley minimum to the entire county. As these instances affect less than four percent of the county-level sample, results are robust to using the county average minimum wage.

[17]As a single county may border multiple counties, γ_c is separately identified from γ_{pt}. Since observations within a county can enter the sample multiple times, all standard errors are clustered at the county level. In general, this approach provides more conservative inference than clustering at the border segment level and does not systematically result in larger or smaller standard errors than two-way clustering by county and year.

sign flexibly captures local business cycle and health outcomes with time fixed effects that vary by county pair, γ_{pt}. This approach may lack external validity, however, as γ_{pt} is only identified for the subset of counties with minimum wages that differ from their neighbors. To allay concerns that facilities in border counties are systematically different from interior county facilities, the Appendix presents results using a state-by-year differences-in-differences framework with Census division-by-period fixed effects and state linear time trends, following Allegretto et al. (2017):

$$y_{fcdst} = \beta log(MW)_{fcdst} + X'_{fcdst}\phi + \gamma_f + \gamma_{dt} + \gamma_s * year + \varepsilon_{fcdst} \tag{1.3}$$

where γ_{dt} is a time fixed effect interacted with Census division and $\gamma_s * year$ is a state-specific linear time trend. The approach in Equation 1.3, while unable to capture granular labor market dynamics, leverages policy variation across all counties. In general, results are robust to this approach, indicating the findings are not due to the unique experiences of border counties.[18]

1.5 Results

Workers

Workers' earnings and employment

Although nursing homes are a large employer of low-skilled labor, the previous literature has not fully examined how minimum wages this labor market in the US context. Therefore, in order to establish whether minimum wages are binding for low-skilled workers in this setting, I assemble staffing and wage data from several sources.

First, I estimate how minimum wages affect employment, earnings, and turnover with quarterly county-level administrative data on employment, earnings, and turnover (hires plus separations) for nursing care facilities (NAICS code 6231) from the Quarterly Workforce Indicators (QWI). Table 1.1 shows that approximately 90 percent of nursing staff are women, about half of nursing assistants have a high school diploma or less, two-thirds of LPNs have some college, and two-thirds of RNs have a four-year college education. As the QWI does not include occupational information, I proxy for minimum-wage nursing staff by focusing on female employment and classifying workers in three education bins: those with no more than a high school education, some college, and a four-year degree or higher.[19]

[18] An alternative approach, coming outcomes among facilities within a Hospital Referral Region (HRR) is shown in Appendix Table A.8, A.10, and A.12. Again, patterns are qualitatively similar for this sample.

[19] Each group with some post-secondary education has average earnings well above prevailing minimum wages. These workers serve as placebo tests. As the minimum wage should not affect earnings in these occupations, a significant positive relationship between minimum wages and higher-skilled earnings would suggest that the empirical framework is capturing general wage growth that is not limited to low-wage sectors. The lowest-education group is defined as the residual from total wages and employment minus counts allocated to workers with at least some college. Given the occupational structure of nursing home

Table 1.3 examines the relationship between minimum wages, earnings and employment for female nursing home staff by estimating the county-pair differences-in-difference approach outlined in Equation 1.2. Column (1) shows a slight increase in earnings and no significant change in employment for the industry as a whole. Examining effects by education category in columns (2) through (4) indicates that the earnings elasticity with respect to the minimum wage for workers with a high school education or less is approximately 0.12. Moving up the skill distribution, there is no significant earnings response for workers with at least some college education (fewer than 4 years in column (3) and a four-year degree or higher in column (4)), indicating this framework is not merely capturing general industry-level wage increases. Panel (b) shows increases in the minimum wage do not significantly affect employment levels for any skill category although I cannot rule out meaningful decreases (or increases).[20]

In order to probe the robustness of these results and address shortcomings of the QWI analyses, Table 1.4 examines earnings responses by occupation and industry with information from other datasets. Specifically, although the QWI has the advantage of providing an administrative measure of earnings at the county-level, one shortcoming is these data do not include workers' occupational or demographic information. Column (1) of Table 1.4 leverages facility aggregates of wage and salarie by occupation from payroll data for facilities located in California and reported to the Office of Statewide Healthcare Planning and Development (OSHPD). As payroll-based wage data are only available for a single state, these estimates are exclusively estimated on the county and city reforms that occurred within California since 2004. Columns (2) and (3) use national household survey from the Current Population Survey Outgoing Rotation Group (CPS-ORG) files in columns (2) and (3), and column (4) reports annual earnings reported in the decennial Census/American Community Survey (ACS). As previous work frequently leverages the CPS to measure the effect of minimum wages on labor market outcomes (Card, 1992; Neumark and Wascher, 2011; Allegretto et al., 2011, 2017; Burkhauser et al., 2000; Clemens and Wither, 2019), these analyses provide a useful benchmark to the existing literature. Since geographic information is not available at the county level for all respondents, the county-pair differences-in-difference approach is not feasible with these data; therefore, I follow Allegretto et al. (2017) and estimate a differences-in-differences model including Census division-by-year and either (CPS) or PUMA (ACS) fixed effects, as in Equation 1.3. Despite slightly different identifying assumptions and analysis samples, results are consistent across data sources, and indicate the elasticity of nursing assistant wages to the statutory minimum ranges between 0.12 (hourly wages in the CPS) to 0.34 (annual wage income in the ACS). The CPS analyses imply somewhat larger earnings reductions for higher-skilled workers than the QWI, but importantly, these wage compressions

staff, the "high school" groups likely include food service and maintenance workers; while the "college" results likely include upper-level managerial staff and physicians. In addition, the QWI data are aggregated at the county level, and cells with fewer than three establishments or three employees are not included in the public-use files.

[20]These general patterns are robust to sample modifications, including limiting the sample to county pairs with centroids less than 75 miles apart or including all genders, and turnover results are nearly identical when restricting the sample to those employed for fewer than three months at their current employer.

suggest the empirical approach is not simply capturing general health sector wage increases.

Table 1.5 brings in additional information on staffing levels and hours worked by occupation at the facility level with information reported to CMS through the Online Survey Certification and Reporting (OSCAR) and Certification and Survey Provider Enhanced Reporting (CASPER) systems (Centers for Medicare and Medicaid, 2018c). Similar to the results in Table 1.3, point estimates in panel (a) of Table 1.5 suggest higher minimum wages do not significantly change the amount of nursing assistant or vocational nurse time per resident (panel (a)). In contrast, there are small reductions in the amount of patient time provided by registered nurses, with a 10 percent minimum wage increase reducing RN time by about 17 seconds a day. Looking at the extensive margin of employment, panel (b) shows slight increases in the number of nursing assistants following minimum wage increases, with no change in the number of vocational or registered nurses. These results are consistent with dynamic monopsony models with search and matching friction where higher wages enable employers to fill vacancies (Manning, 2003; Flinn, 2006). Appendix Table A.3 confirms the slight increase in nursing assistant staff is particularly large for part-time workers (those usually working 35 or fewer hours a week).[21]

To place these results in context with the existing minimum wage literature, I scale the point estimates in Table 1.3 by the fraction of nursing assistants with earnings close to the minimum wage in Table 1.1. This calculation suggests that a 10 percent increase in the minimum wage would increase affected workers' earnings by 3.5 percent, similar to the 3.2 percent for affected food services workers implied by the estimates in Dube et al. (2016) also using the QWI. Figure 1.6 provides a more comprehensive comparison by calculating the earnings (panel (a)) or employment (panel (b)) elasticity with respect to the minimum wage among affected workers from estimates in the previous literature, and plotting the point estimates and standard errors in a funnel plot where more precise estimates are further from the x-axis. The orange markers are the scaled estimates from Tables 1.3 and 1.4 (earnings) and Table 1.3 and 1.5 (employment). Figure 1.6 shows my earnings estimates are similar in magnitude to those found in the previous literature. While there is less of a consensus of the effects of minimum wages on employment, both the QWI and OSCAR/CASPER employment responses are within the range documented in earlier work.[22]

The combination of higher earnings and no systematic reduction in employment levels for low-skilled staff at first appears at odds with limited monopsony power in the nursing assistant labor market found in Matsudaira (2014b). As Matsudaira (2014b) notes, however,

[21]The shift towards part-time work may reflect either firms being more easily able to recruit workers, or employers reducing labor costs by reducing the amount of overtime work. More broadly, the early literature finds mixed results on the relationship between minimum wages and hours worked (Gramlich, 1976; Katz and Krueger, 1992).

[22]Much of the disagreement in the existing literature stems from differences in the empirical approach and time period under observation (Allegretto et al., 2017; Neumark et al., 2014; Cengiz et al., 2019). Appendix Table A.5 shows that minimum wages do not significantly reduce the number of nursing assistants or the hours worked in models leveraging state and year fixed effects, at least since the mid-1990s. These results are consistent with the conclusion reached by Cengiz et al. (2019) that disemployment effects found in early work largely stem from differences in employment trajectories in the 1980s.

there are several reasons why binding minimum staffing requirements do not increase wages *and* binding minimum wages do not decrease employment. First, it is possible that employers respond to both policy changes by altering non-wage compensation or workplace amenities; data limitations preclude a comprehensive evaluation of this channel. Related, if the number of nursing staff per resident measures a job's demands, as in Currie et al. (2005), minimum staffing requirements could be viewed as a form of increasing compensation (by lowering job demands). Second, employment may change on the quality dimension in both cases, with staffing requirements resulting in lower average worker quality and higher minimum wages improving average worker quality. Findings from the previous literature are somewhat mixed on this point. While Matsudaira (2014a) finds no improvement on a variety of patient outcomes, consistent with lower average quality worker offsetting greater staffing numbers, Tong (2011) finds higher staffing levels reduce mortality.[23]

Employee retention and turnover

Although the stock of nursing home workers does not significantly respond to higher wages, flows across employers and between employment and non-employment may change. In job ladder models, turnover is expected to decrease as separations fall due to a lower arrival rate of better-paying jobs, while in frameworks with endogenous separations, separations are expected to increase as fewer employee-employer matches are profitable (Dube et al., 2016; Bontemps et al., 1999; Mortensen and Pissarides, 1994; Pissarides, 2000; Brochu and Green, 2013). On the hiring side, with search frictions or employer bargaining power in the labor market, higher minimum wages can increase the hiring rate (Flinn, 2006; Manning, 2003; Card and Krueger, 2015); on the other hand, hiring may fall if firms find it more costly to post vacancies (Dube et al., 2016). Panel (a) of Table 1.6 shows no significant change in turnover for any worker category.

Disaggregating the turnover rate into hires and separations in panels (b) and (c) shows responses consistent with dynamic monopsony models with search frictions in which higher wages enable firms to more easily fill vacancies for low-skilled work and to retain these workers for longer periods. Panel (b) indicates that "stable" hires, defined as new hires who remain with the same employer for at least three months, increases among low-skilled workers, while panel (c) indicates separations fall for all worker categories. The magnitude of these separation elasticities is similar to the -0.23 found in Dube et al. (2016) for teenagers and restaurant workers using the same data, as well as the elasticities documented for teenagers in Portugal and Canada (Portugal and Cardoso, 2006; Brochu and Green, 2013). Returning to the conceptual framework in Section 1.3, to the extent that firm-specific human capital increases with tenure, greater retention is expected to lower worker effort costs (a downward shift in the $b(.)$ function) and shift the firm's quality-cost function upwards (away from

[23] Another possible explanation is that California nursing assistant labor markets in the late 1990s are not representative of those in other jurisdictions over the full 1990 through 2017 period. At odds with this hypothesis, however, I do not find stronger positive employment responses when dropping either the state of California and/or the early years of the sample period across any staffing measure.

the origin in Figure 1.1, Quadrant III), thereby improving the quality of care provided to residents.

Worker types

Standard efficiency wage models posit that higher earnings can result in higher productivity either by changing the effort levels of those employed, changing the composition of the workforce, or both (Shapiro and Stiglitz, 1984). As critics of minimum wage increases point to potential disemployment effects, particularly for young and inexperienced workers (Congressional Budget Office, 2019; Wascher and Neumark, 2007; Jardim et al., 2018), understanding whether observed consumer benefits are driven by changes in workforce composition is of policy importance.

Table 1.7 extends the ACS and CPS analysis to examine whether higher wages change the types of individuals working in nursing homes. Neither dataset shows an economically or statistically significant change in worker composition, measured by nativity, racial/ethnic group, gender, educational attainment, or household characteristics. Combined with the insignificant changes in worker turnover in Table 1.3 panel (c), these results suggest that any changes in patient outcomes are likely driven by increases in firm-specific human capital and greater worker performance, rather than changes in the (observable) types of workers employed in healthcare support occupations.

Employee effort and absenteeism

Another margin by which higher wage levels could change staffing is by affecting absenteeism or workplace injury. Longer tenures are expected to reduce injury and illness by increasing workers' aptitude and expertise, while in efficiency wage frameworks, higher wages are expected to reduce injuries and illnesses by improving worker caution and by increasing the opportunity cost of time away from work. Nursing homes have high rates of workplace injury relative to other industries: about 11 of every 100 full-time equivalent workers incurred a workplace injury or illness each year, compared to about 4 per 100 FTE for the private sector as a whole (Bureau of Labor Statistics, 2017). Consistent with higher wages increasing worker effort and skill, Appendix Table A.6 shows a 10 percent increase in the minimum wage reducing total workplace injuries and illnesses by about 2.5 percent (2.7 percentage points), with a statistically insignificant and smaller reduction in more severe injuries that require time away from work or a modification of duties.[24]

[24]Improving worker attendance could provide positive externalities to staff whose leave use does not change by reducing the need for overtime or lowering patient-staff ratios. Although data on overtime use is not systematically collected, the common use of overtime in this sector suggests lower absenteeism is an additional channel by which workplace climate could improve, thereby helping firms retain employees.

Consumers

Given that higher minimum wages increase earnings among low-skilled nursing home staff and reduce separations without significantly reducing employment or changing the nursing skill mix, the conceptual framework in Section 1.3 predicts higher minimum wages should improve patient outcomes. To empirically test this prediction, I examine how higher minimum wages affect the consumer experience applying the facility-level county-pairs differences-in-differences design in Equation 1.2 to several objective measures of health and safety.[25]

Patient safety

Nursing homes receive unannounced health inspections every 9-15 months.[26] These inspections are conducted by independent state surveyors who observe the facility and interview staff, patients, and family members about the quality and frequency of care (Abt Associates, 2013; Centers for Medicare and Medicaid, 2018a). The CMS OSCAR/CASPER database includes information on the type, number, severity, and scope of each violation a facility has received. Violation categories include factors associated with patient safety, such as accident hazards and sanitary food preparation, as well as correlates of worker productivity, including conducting routine resident assessments, communicating changes in patient conditions to family members, changing bed linens, and providing each resident with prescribed services.

I compile a database of every violation received by a facility from a state health inspection since 1998 and examine several measures of patient safety: the total number of violations, the total number of severe violations (those presenting immediate harm or danger to residents at the time of inspection), and a standardized measure incorporating the number of violations and the severity and prevalence of each infraction. In particular, CMS assigns a numeric "score" to each violation based on both severity and scope, and I convert this raw score into a standardized measure, following Kling et al. (2007) and Anderson (2008).[27] For each measure, I consider both all violations, as well as a "quality of care" subset that the previous literature has identified as being most closely associated with nursing responsibilities and productivity (Chen and Grabowski, 2015; Harrington et al., 2000, 2001; Matsudaira, 2014a; Antwi and Bowblis, 2018).[28]

[25] All outcomes are weighted by facility size in order to indicate how higher wages affect care in the industry as a whole.

[26] The time between inspection periods is uncorrelated with minimum wages, and inclusion of the number of violations, violation score, presence of a severe violation, facility characteristics, and local labor market controls do not substantially increase predictive power. In the most saturated models with facility and time fixed effects and past violation, county, and resident controls, observable characteristics explain approximately 40 percent of the variation between inspection periods, suggesting that inspection timing is largely random.

[27] For facility f in year y, the raw score is calculated by adding the points assigned to each violation v_{fy} in severity category, sev, and scope category sc: $v_{fy} = \sum_{sc \in SC} \sum_{sev \in SEV} v_{sc,sev,f,y}$. The standardized measure is $z_{fy} = \frac{v_{fy} - \overline{v}}{\sigma_{\overline{v}}}$ where $\overline{v}$ is the grand mean among all nursing homes (pooling all years) and $\sigma_{\overline{v}}$ is the corresponding standard deviation.

[28] Following Harrington et al. (2000), "quality of care" violations are those directly related to resident care

Inspection violations are common in nursing homes: each inspection period, approximately 96 percent of facilities had at least one violation, and the typical facility had five. About a quarter of all citations are due to five quality of care infractions, all of which relate to direct care staff's productivity: improper infection control and prevention, not maintaining accurate medication records, unsafe or unsanitary food preparation and storage, not taking measures to avoid preventable accidents, and not providing basic care.

While inspection violations indicate an environment that jeopardizes patient safety, and therefore health, most incidents are relatively minor in both scope and severity. More than 90 percent of infractions put residents at risk for acute harm, but are not causing harm or immediate danger at the time of the inspection (Appendix Table A.7). These patterns suggest although violations indicate a risky environment, infractions may not immediately translate into patient well-being.

Table 1.8 panel (a) shows that while higher minimum wages do not significantly affect the likelihood that a facility has any violation (column (1)), a 10 percent increase in the real minimum wage reduces the number of violations by approximately 0.06 (column (2)). The final row in each panel of Table 1.8 (ϵ_{mw}) presents the implied elasticity of violations with respect to the minimum wage. By this metric, a 10 percent increase in the minimum wage reduces the number of violations by approximately 1 percent. Although one might be particularly concerned with the most egregious violations, there is no evidence of a systematic reduction in offenses that are currently endangering residents. While column (3) shows an increase in the probability of receiving at least one severe citation, column (4) does not show any significant change in the number of such violations, and incorporating both the prevalence and severity in a standardized score, column (5) suggests a 10 percent increase in the minimum wage improves patient safety by approximately 0.01 standard deviations.

Table 1.8 panel (b) presents analogous results for the subset of "quality of care" violations that are most closely associated with nursing care. Higher wages lead to larger reductions in quality of care violations, with a 10 percent minimum wage increase reducing the likelihood a facility has any quality of care violation by 0.5 percentage points (0.6 percent) and the number of such violations by about 0.07 (2 percent). However, there is no significant change in severe violations, measured by having any severe violation, the number of such violations, or a standardized score.[29]

These findings provide some of the first empirical evidence on how higher minimum wages affect consumer well-being and worker productivity, and provide suggestive evidence

and include infractions in the assessment, quality of care, nursing, dietary, physician, rehabilitative services, dental, and pharmacy regulation categories. I obtain similar results when measuring care violations as the subset of violations that are likely due to one of 33 job tasks or 25 job activities using ONET, as well as the subset that result from patterns of nursing care (in order to proxy more directly for worker productivity). Results and a list of violations under these alternative definitions are available upon request.

[29]For all measures, these findings are robust to alternative weighting schemes and sample definitions, shown in Appendix Table A.8, as well as limiting the sample to substate reforms (thereby accounting for any coincident change in state inspection policy or stringency). In addition, falsification exercises show a slight *increase* in fire code violations unrelated to nursing care and no change in violations for admission practices, suggesting that the results in Table 1.8 are not capturing an overall improvement in facility environments.

higher wages yield better service provision. The existing evidence on this relationship is limited to a single, national minimum wage reform in the UK, where higher minimum wages increased inspection violations (Giupponi and Machin, 2018). Although both British and American nursing homes are labor-intensive firms that employ staff at low wages and have high occupancy rates, these industries differ in their ability to increase consumer prices. UK local governments regulate prices, and reimbursement rates do not account for staffing costs. Therefore, firms may cut costs on other margins in order to maintain profitability. As I document later, American firms are able to fully pass higher labor costs through to consumers and incur no significant change in profitability.[30]

Previous work in public health and economics has examined the relationship between patient safety and other policy-induced changes in nursing home staff, as well as non-wage interventions. This work documents higher levels of firm-specific human capital and greater facility funding reduce the number of violations. On the other hand, the relationship between higher staffing levels and violations less robust (Matsudaira, 2014a). The results in Table 1.8 suggest higher wages are at least as cost-efficient as reforms that increase staffing without changing remuneration. For example, previous work estimates an additional hour of nursing assistant care per patient day (a 44 percent staffing increase for the average facility) is associated with 0.7-1.2 fewer violations (Bowblis and Roberts, 2018; Harrington et al., 2000). Antwi and Bowblis (2018) estimate a 10 percentage point reduction in turnover in all nursing (RN, LPN, and assistant) staff due to poor economic conditions leads to 0.7 fewer quality of care violations. By this metric, a 10 percent increase in the minimum wage is equivalent to a one percentage point reduction in turnover (1-2 percent).[31] Finally, these estimates imply a 10 percent increase in the minimum wage is similar to improvements in patient safety following a four percentage point increase in the unemployment rate (Huang and Bowblis, 2018), or about one-third of the improvements in safety following increased Medicare reimbursement rates in the early 2000s (Konetzka et al., 2004).[32]

Resident health outcomes

Table 1.8 suggests minimum wages improve patient safety along dimensions intended to capture quality of care. However, the lack of a robust relationship between minimum wages and the most harmful violations leaves open the question whether minimum wages translate into measurable health outcomes.

[30] There are other possible explanations for these different findings. For example, the nature of inspections may differ between the countries. In addition, Giupponi and Machin (2018) evaluate a single, national reform over a one-year period where less than 40 percent of firms have an observation both before and after the reform, which precludes fully accounting for changes in inspection routines or the long-term care sector that affected firms at the same time. In contrast, the present analysis period includes up to 20 years of inspection data with minimum wage reforms that affected different facilities at different times.

[31] Estimates of annual nursing assistant turnover range from 62 percent to 86 percent (Berridge et al., 2018).

[32] In the early 2000s, reimbursement rates increased up to 20 percent, depending on RUG group.

To make further traction on this how higher wages for direct care staff affect patients, I examine several measures of resident health that facilities are required to submit to CMS on a quarterly basis. These “Quality Measures” (QM) are conditions that are indicative of the quality of services direct care staff provide (Dorr et al., 2005). Previous work has confirmed additional staff reduce the prevalence of pressure ulcers (Brandeis et al., 1994; Dorr et al., 2005), UTIs (Brandeis et al., 1994; Dorr et al., 2005), physical restraints (Phillips et al., 1996), and psychotropic medications (Cawley et al., 2006; Hughes et al., 2000; Grabowski et al., 2011). Much of this work, however, focuses on credentialed (RN) staffing levels; there is less evidence on the relationship between direct care workers’ compensation and patient health.

First, pressure ulcers (also called pressure sores or bed sores) are a preventable, but relatively common, adverse health condition among nursing home residents. Over the sample period, more than eight percent of residents had a moderate-to-severe ulcer each quarter. By helping residents with mobility and transportation and monitoring health conditions, greater nursing assistant attention is expected to reduce the formation and severity of ulcers.

Consistent with higher wages improving the quality of care, Table 1.9, column (1) shows that a 10 percent minimum wage increase reduces the fraction of residents with moderate-to-severe pressure ulcers by approximately 0.14 percentage points, or about 1,900 fewer cases each reporting period. This result is unchanged with the inclusion of time-varying resident demographic characteristics in column (2), suggesting that the reduction is not driven by facilities admitting a greater share of low-risk patients.

Second, UTIs are the most common bacterial-related cause of hospitalization, and a common infection among long-term care residents, affecting about seven percent of residents (Genao and Buhr, 2012). Indwelling catheters, administered and monitored by nursing assistants and support staff, are a common cause of UTIs and prompt removal or reduced catheter use reduces the risk of infection (Genao and Buhr, 2012; Saint, 2000; Matsumoto, 2001; CDC, 2009; ONET, 2018). Although point estimates in columns (3) and (4) suggest higher minimum wage slightly reduce the share of residents with infections, this relationship is not statistically significant.

Third, nursing homes may adjust the use of physical restraints in response to greater staff-patient attention or resources; however, the direction of this relationship is ambiguous. If restraints require intensive staff attention or assembly, higher wages may increase the use of such devices (Grabowski et al., 2011), but as physical restraints restrict movement, greater nursing resources and attention should reduce the use of these devices (Cawley et al., 2006). Columns (5) and (6) shows a weak negative relationship between minimum wages and physical restraints, consistent with other work finding higher staffing levels are associated with fewer restraints (Cawley et al., 2006; Phillips et al., 1996; Grabowski et al., 2011).

Fourth, psychotropic medications are drugs that have sedating properties and affect mental processes and behavior; accordingly, higher quality care is expected to reduce the fraction of residents receiving these medications.[33] While previous work has documented

[33]Psychotropics include anti-psychotics, antidepressents, anxiolytics, and hypnotics. These medications

additional licensed nursing staff are associated with lower anti-psychotic use (Hughes et al., 2000; Grabowski et al., 2011), columns (7) and (8) indicate that higher minimum wages do not reduce the use of psychotropic medications, with point estimates suggesting a meaningful increase.

Finally, columns (9) and (10) of Table 1.9 combine results for pressure ulcers, UTIs, and physical restraints into a standardized "poor health outcome" index, defined so that a value of one equals a one standard deviation worsening of composite patient outcomes (Kling et al., 2007; Anderson, 2008). As suggested by the individual point estimates in columns (1) through (6), higher minimum wages improve patient outcomes: a 10 percent minimum wage increase improves patient outcomes by 0.02 standard deviations.

These results indicate that modest minimum wage increases yield meaningful improvements in consumer health for conditions that result from patterns of care. The estimated improvements shown in Columns (1) and (2) of Table 1.9 are larger than those from similar-costing policies that are often promoted in an effort to improve nursing home care. In particular, minimum staffing requirements and unionization have no robust effect on pressure sores prevalence (Matsudaira, 2014a; Sojourner et al., 2015), and a 10 percent minimum wage increase reduces ulcers by a rate similar to a near-doubling of RN care (Konetzka et al., 2008; Dorr et al., 2005).

The reductions in pressure ulcers is also sizable relative to improvements in resident health stemming from business cycle fluctuations. For example, a 10 percent increase in the minimum wage is approximately equivalent to improvements in patient outcomes following a 1.2 percentage point increase in the local unemployment rate (Huang and Bowblis, 2018) or a 2.6 percentage point (approximately 3-4 percent) reduction in staffing turnover (Antwi and Bowblis, 2018).

Mortality

The results in Tables 1.8 through 1.9 are overall consistent with minimum wages improving patient care. Inspection violations, pressure ulcers, and other assessment-based measures of patient health are objective quality measures; however, each of these outcomes may be subject to concerns about measurement error. For example, about half of UTIs are asymptomatic, inspections are prone to oversight on the part of inspectors, and resident health status is reported by facility employees.[34] Death, although an extreme negative outcome, is well-measured and not subject to these concerns. Although mortality rates are relatively low in the general population, rates are relatively high among nursing home residents: about one-third of residents die within a year of admittance, about three times the overall death

were introduced as a quality measure in 2011, somewhat restricting this analysis.

[34] If minimum wages induce greater worker effort or reduce cognitive pressures, these assessments are expected to become more accurate following minimum wage increases. Under such "ascertainment bias," higher wages are expected to increase the number of residents *reported* as experiencing one of these conditions, and the results in the previous section represent a lower bound on patient health improvements (Arling et al., 2005).

rate for the population ages 85 and older (Flacker and Kiely, 2003; National Vital Statistics Reports, 2018).

To examine the relationship between higher minimum wages and mortality, I obtain information on the annual number of deaths by age and county and place of death from Vital Statistics microdata.[35] Over the analysis period, the elderly subpopulation became more aged. As mortality rates are sharply increasing in age, I age-adjust the annual elderly mortality rate, m_{cy}, based on each county's single-year age distribution in the year 2000:[36]

$$m_{cy} = \sum_{a=65}^{85+} \frac{deaths_{cay}}{pop_{cay}} * \frac{pop_{a,2000}}{\sum_{k=65}^{85+} pop_{k,2000}} \tag{1.4}$$

Where $deaths_{cay}$ is the number of deaths in nursing homes in county c among individuals aged a in year y, and pop_{cay} is the number of individuals aged a each county-year.[37] $\frac{pop_{a,2000}}{\sum_{k=65}^{85+} pop_{k,2000}}$ is the national fraction of individuals age a in the elderly population in year 2000.

Table 1.10 shows the relationship between minimum wages and age-adjusted elderly mortality rates by place of death.[38] A 10 percent increase in the minimum wage reduces the overall elderly mortality rate by 0.7 percent (column (1)), or 0.5 percent accounting for demographic changes in nursing home residents. This overall increase in longevity is driven by lower mortality in nursing home settings. A 10 percent increase in the minimum wage reduces deaths in the nursing homes by 3.3 percent, or 3.1 percent controlling for changes in resident demographics. Applying the estimate in columns (3) and (4) to the number of nursing home deaths in 2013 (approximately 488,000) suggests an across-the-board 10 percent increase in each county's minimum wage that year would have saved approximately 15,200 to 16,200 lives.[39] In contrast, there is no significant relationship between minimum wages and elderly mortality rates in non-nursing home settings (columns (5) and (6)), including deaths occurring in hospitals (columns (7) and (8)).[40]

[35]This analysis covers the 1990-2013 period and focuses on deaths in "nursing homes" for years prior to 2003 and "nursing home/long term care" settings in subsequent years. Following Stevens et al. (2015), I confirm this slight definition change does not coincide with a series break in the share of deaths occurring in nursing homes.

[36]Appendix Figure A.3 compares the elderly age-adjusted and raw mortality rate over the study period.

[37]This measure understates the mortality rate among nursing home patients as approximately 20 percent of residents die after being transferred to a hospital (Temkin-Greener et al., 2013). This undercount presents problems for a causal interpretation of the results if minimum wage reforms coincide with changes in facility discharge or hospital admittance policies. However, I find minimum wages do not affect hospitalization rates or the elderly mortality rate in hospitals.

[38]As county-year population data are top-coded at age 85 and approximately 40 percent of nursing home deaths occur among older than 85, I am unable to determine how minimum wage increases affect life expectancy.

[39]Additional results suggest nursing home mortality reductions are concentrated among deaths due to respiratory conditions, degenerative brain diseases, and kidney-related conditions. For deaths outside nursing homes, only kidney conditions is significantly correlated with minimum wage.

[40]Results are qualitatively robust to alternative measures of mortality, including inverse hyperbolic sine, the number and log number of deaths and the mortality rate expressed in levels.

The magnitude of these changes is large relative to the modest costs of minimum wage increases. For example, Stevens et al. (2015) estimate a one percentage point increase in a state's unemployment rate reduces nursing home mortality rates by 4.7 percent. By this metric, a ten percent increase in the minimum wage is equivalent to a 0.66 percentage point increase in the unemployment rate. Put another way, the 2007-2009 increase in the federal minimum wage had approximately half the estimated effect on elderly mortality as the 5.6 percentage point change trough-peak change in national unemployment experienced during the Great Recession.

These mortality reductions are also large relative to the estimated effects of other policy changes affecting nursing home labor markets. For example, Tong (2011) finds California's minimum staffing requirement increased staffing by ten percent and reduced mortality in affected facilities by 4.7 percent. As a ten percent increase in minimum wages increases nursing assistant wages by approximately one percent (Table 1.3), this comparison suggests increasing wages may be more cost-efficient than increasing staffing levels alone.

Patient safety and health: Robustness and alternative explanations

Alternative empirical approaches

The main analyses leverage minimum wage differences between neighboring counties and control for county unemployment rates in order to isolate wage variation that is orthogonal to local labor market conditions.In order to allay concerns that these results are sensitive to the county-pair design or sample, Appendix Tables A.9 and A.11, and columns (1) and (2) of Appendix Table A.12 present results for nursing homes in all counties, including those in the interior of policy boundaries by replacing county pair-by-time fixed effects with Census division-by-time fixed effects and state-specific linear trends using the approach outlined in Equation 1.3. While these specifications cannot fully account for more local changes that may affect the minimum wage and elderly health, despite somewhat stronger identifying assumptions the main results are largely robust to this modification.[41]

In additional robustness checks, Appendix Tables A.8 and A.10 show the main patient safety and health outcomes are qualitatively unchanged with slight sample modifications: including extreme values, unweighted specifications, and omitting facilities that are located within hospitals. Finally, panel (d) of Appendix Tables A.8 and A.10 and column (3) of Appendix Table A.12 replaces the county-pair sample with the set of facilities (violations and patient health) and counties (mortality) that are located in a Hospital Referral Region

[41] While the point estimate on nursing home mortality is slightly positive in the all county sample when state-by-year trends are included (Appendix Table A.12), there is an active debate in the literature on the appropriate use of geographic controls; moreover, it is unclear whether the rationale for including trends when measuring labor market variables pertains to other outcomes. Models with county and year fixed effects (and the standard labor market, demographic, and policy controls) produce results nearly identical to those in Table 1.10, column (4).

(HRR) where the statutory minimum wage differs across jurisdictions. Compared to the county-pair sample, the HRR sample includes more rural and midwestern counties (Appendix Figure A.4), but improvements in patient health are similar to the main findings. Finally, in results available upon request, results are not driven by the early or later years of the analysis period, and are qualitatively similar when limiting the sample to within-state minimum wage changes (accounting for all observed and unobserved changes in state-level income assistance, Medicaid, and labor market policies).

Dynamic responses to minimum wage reforms

The differences-in-differences analyses show the contemporaneous effect of minimum wage increases. If safety measures take time to implement or infections and mortality result from patterns of care over a longer time horizon, the medium-term effect of higher wages will be larger than the immediate effect. On the other hand, minimum wage changes are announced several months prior to the effective date, and firms may adjust wage schedules before the reform becomes effective. More generally, a causal interpretation of the differences-in-differences results relies on the assumption that within a pair of neighboring counties, the timing of minimum wage changes is uncorrelated with factors affecting elderly health.

In order to provide visual evidence whether this assumption is reasonable, Figure 1.7 presents event study plots showing changes in minimum wages (blue line, hollow circles) and patient outcomes (red line, solid circles) before and after the wage increase. I focus on the number of quality of care violations (panel (a)); the prevalence of pressure ulcers (panel (b)) and UTIs (panel (c)); and nursing home mortality (panel (d)), and track outcomes over a 13 quarter (panels (a) through (c)) or ten year period (panel (d)). Since minimum wage changes are frequent events, and can occur in either county of the county pair, I limit the event study sample to reforms that increased the inflation-adjusted minimum wage gap between the two counties by at least five log points, and exclude events that followed other reforms that changed the log gap by more than 0.5 log points in the previous six quarters (panels (a) through (c)) or four years (panel (d)). Appendix Figure A.5 shows the years in which these events occurred for each outcome.[42] Although a relatively small fraction of all reforms in the main analysis, these changes represent the cleanest breaks from the status quo. I then scale each event by the size of the treatment, following an approach similar to that in Dow et al. (2019) and Finkelstein et al. (2016) and stack all events.[43] Specifically, I estimate for outcome y_{fcpyq} in facility f in county c in year y in quarter q:

$$\begin{aligned} y_{fcpyq} = \sum_{i=m}^{n} \kappa_i \mathbb{1}\{\gamma_{yq} = i\} * \mathbb{1}\{\Delta log(MW)_{cpi=0} > \Delta log(MW)_{(-c)pi=0}\} * \\ \big(\Delta log(MW)_{cpi=0} - \Delta log(MW)_{(-c)pi=0}\big) + X^{`}_{cpyq}\phi + \gamma_{py} + \gamma_f + \gamma_{i=0} + \varepsilon_{fcpyq} \end{aligned} \tag{1.5}$$

[42] Given different reporting windows, the sample of minimum wage reforms slightly varies across outcomes.
[43] A distributed lag specification yields qualitatively similar, albeit less precise, results.

where $\mathbb{1}\{\gamma_{yq} = i\}$ is an indicator function for each quarter (in event time i), interacted with an indicator for facilities on the "treatment" side of the change, $\{\Delta log(MW)_{cpi=0} > \Delta log(MW)_{(-c)pi=0}\}$ (those that experienced a larger change in the minimum wage than their neighbors), scaled by change in the log county pair minimum wage gap $(\Delta log(MW)_{cpi=0} - \Delta log(MW)_{(-c)pi=0})$. As in Equation 1.2, the X vector includes county unemployment rates and facility and population characteristics, as well as quarter fixed effects γ_q for the results in panels (a) through (c). γ_{py}, and γ_f are county-pair-year and facility fixed effects, respectively, and $\gamma_{i=0}$ is an additional fixed effect for each reform.[44] The mortality specifications simply replace facility fixed effects with county fixed effects and define event time in years, rather than quarters.

Crucially, Figure 1.7 does not show evidence of economically or statistically significant pre-trends for any outcome, supporting a causal interpretation of the main results, and suggesting that the estimates in Tables 1.8, 1.9, and 1.10 are not simply picking up correlations between longer-term improvements in elderly health and prevailing wages.

These figures also illustrate the timing of changes in patient health. While the effect on pressure ulcers dissipates within five quarters, reductions in mortality persist for up to three years. These patterns are consistent with the nature of each outcome: inspection citations occur immediately but may also reflect longer-term environmental features, UTIs can develop within several days, whereas pressure sores more commonly result from persistent lack of movement over several weeks or months, and mortality is the result of cumulative health inputs.

Finally, the estimated improvements in patient outcomes are similar in magnitude to the main differences-in-differences results, albeit less precisely estimated. In particular, panels (a) through (c) show a small reduction in the number of violations (averaging -1.3 for a 100 log point minimum wage increase), pressure ulcers (-5.2 percentage points), and UTIs (-1.4 percentage points) over the subsequent six quarters. In addition, panel (d) suggests mortality rates fall by approximately 20 to 30 log points following a 100 log point change in the minimum wage gap, similar to the results in columns (3) and (4) of Table 1.10.[45]

Patient composition

The main results control for resident demographic characteristics in order to avoid confounding changes in patient outcomes with facilities' underlying risk factors, and results are similar when excluding these controls. However, examining changes in the types of individuals with access to residential health services has social welfare implications. As illustrated in Section

[44] γ_{py}, $\gamma_{i=0}$, and $\mathbb{1}\{\gamma_{yq} = i\}$ are separately identified as county pair-years can contribute to multiple reforms.

[45] Although each of these plots suggests improvements in health larger than the results in Tables 1.8 and 1.9, the event study framework only leverages the largest and most temporally isolated minimum wage increases – reforms expected to generate the largest changes in patient outcomes. Additional results, available upon request, provide suggestive evidence that improvements in care exhibit diminishing marginal benefits with respect to the size of the minimum wage change.

1.3, establishments may offset higher labor costs by increasing prices or targeting private-paying residents, who tend to be relatively wealthy.[46] In addition, Medicare rates depend on care needs and rehabilitation use, with higher-need patients providing more generous reimbursement (summarized by a Resource Utilization, or RUG, group). These patterns suggest profit maximizing firms may target Medicare and private payors when factor prices increase in order to increase revenue and avoid reducing staffing levels.

Consistent with this hypothesis, Table 1.11 shows a ten percent increase in the minimum wage reduces the the share of residents covered by Medicaid by 0.5 percent (0.26 percentage points, column (1)), while the share of residents paying out-of-pocket or covered by private insurance increases by a similar amount by (0.25 percentage points, or one percent, column (2)), and the fraction of admitted residents covered by Medicare does not significantly change (column (3)).

Facilities may adjust their revenue by classifying residents as requiring more intensive care in order to receive greater revenue from Medicare and private patients. Evidence on this margin is somewhat mixed, and sensitive to the population examined.[47] Across all residents, average care needs increase about 0.02 standard deviations following a 10 percent minimum wage increase (Table 1.11 columns (4) and (5)).[48] Although the available data do not allow a full disaggregation of what types of residents account for these changes, the observed shifts are not driven by Medicare recipients, as there is no economically or statistically significant change in average Medicare reimbursement rates (Columns (6) and (7)). In principle, however, higher reimbursement rates correspond to higher per-patient staffing time and costs, and if the Medicaid formula for each need group is accurate (and each patient accurately categorized), higher-need patients may bring greater revenue but these additional resources are offset by higher costs.

Columns (8) through (10) of Table 1.11 examine whether firms respond to higher labor costs by changing their discharge or admission practices. For example, higher wages may incentivize firms to discharge relatively low-revenue Medicaid patients, to leverage economies of scale by increasing their occupancy rates, or to increase their transfers to hospital settings in order to receive Medicare revenue once patients are readmitted to the facility (Mor et al., 2010). Columns (8) and (10) show no economically or statistically significant changes in the discharge or occupancy rates, respectively. Consistent with improved resident care and inconsistent with facilities "churning" patients to maximize revenue, column (9) shows a ten percent increase in the minimum wage decreases hospital admissions by 0.13 percentage points (0.8 percent).

Finally, Table 1.12 examines other changes in resident characteristics and shows a slight

[46] Appendix Table A.1 shows that average Medicaid reimbursement rates are approximately 26 percent lower than rates received by private payors, and about half that of average Medicare reimbursement.

[47] Analyses for Medicare recipients are further complicated by periodic changes in the reimbursement schedule, the most substantial of which occurred in 2011.

[48] These composite indices are computed from indices summarizing the number of ADLs residents require assistance with (column (1)) and the number of ADLs and additional therapeutic and rehabilitative services (column (2)).

decrease in the share of female residents, but no other significant changes along observable dimensions.

In order to place bounds on the extent to which changes in resident composition drive the main findings, I estimate Equation 1.2 on the predicted changes in patient health exclusively due to changes in demographic and payment composition. For each outcome y, I calculate the predicted measure of health at time t, $\widehat{y_{ft}}$, from a third-order polynomial of the Medicaid share and each variable in Table 1.12:

$$\widehat{y_{ft}} = \sum_{i=1}^{3} \left(\beta_{1i}\overline{age}^{i}_{ft} + \beta_{2i}\%female^{i}_{ft} + \beta_{3i}\%black^{i}_{ft} + \beta_{4i}\%white^{i}_{ft} + \beta_{5i}\%Mcaid^{i}_{ft}\right) \quad (1.6)$$

The results in Appendix Table A.13 show changes in resident characteristics only significantly reduce violations, and the last row of the table indicates these changes only account for at most 7-12 percent of the observed improvements in patient health and reductions in mortality.[49]

Although changes in resident composition do not drive the main results, these shifts raise distributional considerations. In particular, health conditions for individuals outside of residential settings are not routinely monitored. Results in Table 1.10 do not show a significant increase in elderly mortality occurring outside of nursing homes (columns (3) and (4)). These patterns suggest, at least on the mortality dimension, changes in resident composition does not lead to substantially worse outcomes for those losing access to care.

Heterogeneity

The effects of higher wages on patient outcomes likely depend on the market structure in which providers operate, and the average effects may mask heterogeneous responses across facilities. Several features of the nursing home industry suggests providers may not respond uniformly to minimum wage increases.

First, unlike many other low-wage industries, the supply of nursing home services – both the number of beds within a facility and the number of facilities – is largely fixed by state regulation. In addition, consumer demand is relatively inelastic, as few substitutes exist and residents require routine assistance caring for chronic conditions or performing daily activities.

Appendix Figure A.6 shows the Medicaid share distribution by establishment and illustrates that Medicaid covers the majority of patient-days in most facilities, particularly in private and government-owned facilities. As outlined in Section 1.3, Medicaid recipients, who incur no out-of-pocket cost for care, are particularly likely to have nearly perfectly inelastic demand. In contrast, private payors' demand is increasing in quality and decreasing in price. These clients may respond to higher prices by transferring facilities or seeking less-expensive, community-based care options. In the cross-section, facilities with a higher Medicaid share

[49] In additional results, I do not observe significant changes in resident sorting across facilities (across payment source or demographic characteristics) within the same county, measured by a dissimilarity index.

tend to be lower quality, as measured by greater use of anti-psychotics, higher prevalence of pressure ulcers and physical restraints, and lower staffing levels (Mor et al., 2004). I investigate whether changes in the quality of care are greater among facilities with a larger share of Medicaid residents by partitioning the sample at the median of each facility's maximum observed Medicaid share (77 percent).

Second, provider incentives may vary based on firm ownership. Approximately 70 percent of facilities in the analysis sample are privately owned and a slight majority are part of multi-establishment chains. Previous work has found privately-owned nursing homes, particularly large chains, have lower staffing and more inspection violations than government-owned facilities (Harrington et al., 2012; Hillmer et al., 2005; Grabowski and Stevenson, 2008; Comondore et al., 2009; Cohen and Spector, 1996; Harrington et al., 2001, 2004; Kim et al., 2009; Government Accountability Office, 2009), and other work suggests that non-profit status may itself signal quality (Hirth, 1999; Jones et al., 2017).

Third, responsiveness to minimum wages may depend on the local market structure. Firms operating in markets with few other providers are largely shielded from competitive pressures and have weak incentives to improve quality or attract workers. I empirically explore whether responses are larger in competitive care markets by calculating a bed-count Herfindahl index (HHI) at the commuting zone (CZ) level and defining competitive markets defined as those with a HHI less than 1,500 (on a 10,000 point scale). Appendix Figure A.7 shows the overwhelming majority of facilities operate in nearly-perfectly competitive markets, but a small number, about 100, are the sole provider in their commuting zone.

Fourth, wage regulations may interact with other staffing requirements. Between 1990 and 2010, the number of states with minimum direct care staffing requirements increased from eight to 36 (Harrington, 2010). Staffing levels tend to be higher in states with direct care requirements, shown in Appendix Figure A.8, panel (a). However, in states with requirements, there is no significant bunching at the state-specific minimum threshold (panel (b)), suggesting in practice, these regulations do not bind in most facilities.[50]

Table 1.13 interacts minimums wage with high-Medicaid share, private ownership, chain status, industry concentration, and quantity regulations for each main patient safety and health measure: the total number of care violations (panel (a)), the fraction of residents with pressure ulcers (panel (b)), and the mortality rate (panel (c)). This table shows health improvements do not systematically vary with provider type, suggesting that changes in patient health are not limited to a particular type of facility or healthcare service area.[51]

Appendix Table A.14 extends this framework to the QWI county employment results by estimating the share of facilities in each county with a high Medicaid share, private ownership, and chain status and interacting this share with the prevailing minimum wage. Consistent with Table 1.13, Appendix Table A.14 generally shows that employment and

[50] Density tests around the discontinuity (McCrary, 2008) were conducted following the approach in Cattaneo et al. (2018) with a third-order polynomial.

[51] Appendix tables show that results do not meaningfully change when excluding facilities operating in hospitals, consistent with limited heterogeneity in providers' responses.

earnings responses do not significantly differ along these characteristics, except earnings are more muted in counties with a sizable presence of multi-establishment chains.

Firm costs, revenue, and profitability

If consumer demand is price inelastic, firms will be able to pass costs through to consumers in the form of higher prices. In such a setting, higher minimum wages will increase worker earnings without substantially lowering employment or profits. Even if firms are unable to adjust their pricing strategy, they may be able to offset higher labor costs elsewhere in their balance sheet by selling assets, lowering investments, or increasing their liabilities.[52]

I examine how higher labor costs affect prices charged to residents and overall firm revenue using annual cost report data for the subset of facilities serving Medicare patients in Table 1.14.[53] Column (1) shows costs per resident increase when minimum wages increase. The magnitude of this change is slightly less than half the estimated wage increase in Table 1.3. Applying the point estimates for nursing assistant wages from Tables 1.3 and 1.4 to nursing assistants' share of the labor bill suggest between 70 and 90 percent of the increased costs are due to higher nursing assistant salaries. When maintenance and food preparation staff are added to the QWI and CPS estimates, higher labor costs mechanically account for 97 to 98 percent of the estimated total cost increase, suggesting little scope for factor substitution in this industry.

Column (2) of Table 1.14 suggests that the prices charged to residents increased by more than total cost per resident, although this estimate is imprecise. The amount charged does not always reflect the payment the facility receives, particularly for individuals with insurance coverage, and column (3) indicates revenue per resident increase about 0.7 percent when the minimum wage increases 10 percent. The point estimate in column (3) is slightly larger than the estimated increases in per-resident costs in column (1), suggesting that firms are able to fully pass higher labor costs through to customers. Accordingly, profitability, measured by net income does not significantly change (column (4)).[54]

Changes in per-resident revenue are a combination of changes in patient composition and changes in revenue from each payor type. Column (3) of Table 1.11 showed the fraction of residents not covered by Medicaid or Medicare increases about 0.25 percentage points (1 percent) when the minimum wage increases 10 percent. Among residents who are covered by

[52] In Figure 1.2, such changes are illustrated as a transformation of the quality-cost curve in Quadrant III. If firms were previously operating with the cost-minimizing combination of inputs for a given output level, any changes in production technology will shift the quality-cost curve towards the cost axis, but with substitution across inputs, this shift will be smaller than that implied by a change in wage costs alone.

[53] These data are only available for Medicare-certified SNFs and are not available for facilities that do not serve Medicare patients or those located within hospitals. Further, not all facilities report all information as those with few Medicare patients may submit an abbreviated form.

[54] Net income is defined as all sources of revenue, including those of ancillary, outpatient, and clinical services, minus total costs. The ability of firms to fully cover higher costs through greater revenue holds when limiting the sample to relatively large within-pair changes in the minimum wage (10-20 log points or more).

government sources, however, there is no significant change in per-resident revenue (Columns (6) and (7) of Table 1.11 for Medicare recipients at the firm level and Column (5) of Table 1.14 provides estimates for state average Medicaid per diem).[55] Decomposing the increase in average per-resident revenue into changes in the patient mix and changes in the rates paid by each type of payor indicates that most of the increase in revenue – approximately 75 percent – is due to changes in the prices paid by private payors, rather than reductions in access for government beneficiaries.[56]

A final potential confounding factor is that higher labor costs may cause low-performing firms to exit the market. As outcomes are not available for closed facilities, high rates of firm exit would suggest the previous analyses overstate the aggregate benefits from minimum wage reforms. To explore this issue, I extend the event study analysis from Equation 1.5 on a balanced panel of firms that appear at any point in my sample, with the outcome of interest an indicator for whether the firm exists in a given year. Reassuringly, Appendix Figure A.9 shows there is no change in firm exits or entry for up to four years following a minimum wage increase, indicating that higher minimum wages do not change market composition. This finding stands in contrast to higher rates of establishment entry and exit the fast-food industry (Aaronson et al., 2018), but is consistent with nursing homes operating in markets where supply is constrained and demand for services is high.

The results in Table 1.14 raise the question why firms do not unilaterally increase wages in order to provide higher-quality services. This question is particularly relevant given that firms report difficulty in finding and retaining qualified workers.[57] Several considerations suggest that it may be rational for firms to pay low wages and offer relatively low quality of care. First, Table 1.14 shows that firm profitability does not significantly change following modest minimum wage increases – while firms are not strictly worse off paying higher wages, nor are they better off. Second, with asymmetric information, collective action problems may play a critical role. For example, even if wages were a perfect signal of facility quality, it is unclear prospective residents know or are able to act on this information as most nursing homes operate near capacity or maintain waitlists. By a similar argument, current residents may be unable to change facilities once they have been in residence and realized a firm's quality. In this environment, wage increases by a single firm increase the firm's labor costs, but are accompanied by a muted demand response. In contrast, if all firms are required to increase wages, even if consumers cannot discern a particular firm's quality, they may expect quality at *any* facility in an area will be better after the minimum wage increase than before.

[55]Facility-level Medicaid payments are not systematically collected across states over time. The results in column (5) provide average state payments, collected by Brown School of Public Health (2019) from a survey of state Medicaid officials for the 2000 through 2009 period.

[56]Information on charges, costs, and revenues by payor type are not available at the facility level for most firms. To decompose the changes in resident revenue, I apply the point estimates in Column (4) of Table 1.14 and Columns (1) through (3) of Table 1.11 and approximate prices paid by each payor type as the mark-up implied by Table A.1, applied to the average daily cost in Column (4) of Table 1.14.

[57]On media reports describing difficulties finding workers, see `https://www.nytimes.com/2018/07/27/health/medicare-nursing-homes.html` and `https://www.wgbh.org/news/local-news/2019/10/10/cape-cod-nursing-homes-suffer-from-a-shortage-of-nursing-assistants`.

Therefore, economy-wide wage reforms may be necessary to trigger a meaningful increase in consumer demand that allows firms to operate without lowering profitability.

1.6 Conclusion

This chapter finds that higher wages among low-skilled health workers translate into improved patient safety, better health, and reduced mortality for nursing home residents. These benefits are both statistically significant and economically meaningful. To quantify the magnitude of these changes, I apply the expected costs of pressure sore treatment from the previous literature (Dorner et al., 2009; Meddings et al., 2015; Agency for Healthcare Research and Quality, 2016; Brem et al., 2010) to the point estimates in Columns (1-2) of Table 1.9 and the estimated increase in nursing assistant wages in Tables 1.3 and 1.4 from average annual nursing assistant wages. This simple back-of-the-envelope calculation suggests that cost savings pressure sore treatment alone offset between approximately 20 and 50 percent of the increase in staff costs.[58] Although there is wide variation in valuations of life at older ages (see, for example, Murphy and Topel (2006)), wage increases fully pay for themselves in this sector if the value of increased longevity for nursing home residents is at least $21,000.[59]

Accounting for improved service quality enhances the desirability of minimum wages relative to a framework that does not account for this margin. It is less certain, however, whether minimum wages are socially beneficial. Appendix A.3 outlines the social welfare considerations of higher minimum wages in a society with firm owners and two generations, young and old, each with two income levels, poor and rich. In this simplified economy, young, poor workers earn minimum wages; old poor individuals receive nursing home care covered by Medicaid; young, rich workers earn wages higher than the minimum wage and pay taxes to finance Medicaid; and elderly, rich individuals pay for nursing home care from accumulated assets and leave bequests to their heirs. Higher minimum wages are more desirable the greater the welfare weights assigned to elderly individuals and low-income workers, the higher the dependency ratio of the Medicaid population to current taxpayers, and the responsiveness of nursing home quality to higher wages.

The results documented in this chapter show that higher minimum wages can improve consumer well-being. These findings are also consistent with recent work documenting higher productivity in the retail sector can offset the labor costs of higher wages with no net change in firm profitability (Coviello et al., 2018). When extrapolating these findings to the broader economy, however, several points should be kept in mind. In particular, restrictions on the total supply of nursing home services and regulations on operating procedures create

[58] As I do not find significant changes in employment or consistent changes in earnings for other skill categories, this estimate assumes a null employment response and no changes in earnings among higher-skilled nursing staff.

[59] This estimate is well below reasonable parameter estimates in Murphy and Topel (2006) and Hall and Jones (2007), as well as willingness to pay, as measured by annual costs of residential care (Appendix Table A.1).

entry costs that stifle competition in both the labor and product markets. Second, the lack of close substitutes for nursing home care, combined with government subsidies on the cost of care, suggests consumer demand is relatively inelastic. Therefore, while I do not find significant reductions in firm profitability or employment, these null results may not apply to other industries facing greater competitive pressures or more elastic consumer demand. With these caveats in mind, these results are of policy interest in their own right. In developed countries, including the United States, the government is a major actor in health care provision and financing. For example, Medicaid and Medicare account for nearly 18 percent of US GDP, ten percent of which is spent on long-term care (Centers for Medicare and Medicaid, 2018b). These costs will increase as population ages. The potential to increase longevity and reduce expenditures on preventable medical care through policies that benefit workers in this industry has important social welfare and fiscal implications.

1.7 Tables and Figures

Table 1.1: Earnings and Demographic Characteristics of Nursing Home and Low-wage Workers

	(1)	(2)	(3)	(4)	(5)	(6)
	Nursing home workers			*Other low-wage sectors*		*All*
	Nursing assistants	LPNs	RNs	Food services	Retail	Private sector
Minimum wage exposure						
Workers (1000s) (OES)	604.4	210.9	153.1	12856.1	14452.6	122999.2
Median wage (OES)	13.33	$22.91	$30.98	$11.05	$13.54	$18.58
% affected by MW increase (CPS)	0.336	0.103	0.031	0.653	0.433	0.262
Demographics (CPS)						
% ages 25-54	0.632	0.686	0.720	0.500	0.548	0.647
% HS education or less	0.482	0.229	0.014	0.571	0.427	0.355
% some college, < 4yr degree	0.416	0.674	0.331	0.314	0.348	0.286
% black	0.348	0.273	0.119	0.123	0.133	0.110
% Hispanic	0.157	0.117	0.067	0.256	0.175	0.169
% white	0.419	0.537	0.707	0.521	0.606	0.639
% female	0.887	0.895	0.895	0.514	0.584	0.463
% HH w/ children	0.507	0.553	0.523	0.315	0.347	0.437
% married	0.377	0.483	0.625	0.288	0.413	0.543

Notes: Table shows average wages and demographic characteristics for nursing home workers by occupation (columns 1-3); workers in other low-wage sectors (columns 4-5); and all workers (column 6). The number of workers and median wages is provided from 2018 Occupational Employment Statistics data; the remaining rows are author's calculations using the 2014-2018 CPS-ORG.

Table 1.2: Nursing Home and Area Characteristics, Differences between County Pairs

	(1)	(2)
	Average, highest MW county	County pair difference
Min wage (2017 $)	8.297	0.565***
		(0.085)
Cty unemployment (x100)	6.772	0.128
		(0.114)
Share popn > 65 (x100)	13.93	0.039
		(0.165)
State EITC rate (x100)	7.76	-0.005
		(0.014)
Any state EITC	40.6	-0.024
		(0.036)
TANF/AFDC maximum	560.2	-5.964
		(15.99)
Avg facility size	106.3	-1.038
		(1.444)
CZ HHI (X.0001)	59.66	0.008**
		(-0.004)
% NH residents female (x100)	68.85	-1.155***
		(0.404)
% NH residents black (x 100)	17.57	1.893
		(1.357)
% NH residents Medicaid (x 100)	59.66	0.733
		(0.725)
Avg NH resident age	80.17	-0.766***
		(0.209)
Observations		841958

Notes: Table shows the average characteristics of the county in each county-pair with the highest minimum wage (column (1)) and difference in average characteristics between the highest-minimum wage jurisdiction and lowest-minimum wage jurisdiction within a county-pair year from a regression including a series of year fixed effects and an indicator for whether the facility faced the pairwise-highest minimum wage (column (2)). Sample is limited to county-pair-years with within-pair minimum wage variation. Robust standard errors clustered by county in parentheses. See text for details.

Table 1.3: Minimum Wages and Employment: Quarterly Workforce Indicators

	(1)	(2)	(3)	(4)
	Panel a: Log(earnings)			
	All	<=HS	SC	BA
log(MW)	0.070**	0.117***	0.040	0.052
	(0.031)	(0.031)	(0.032)	(0.037)
N	23214	23214	23214	23214
DV mean (level)	2622.8	2088.2	2838.0	3422.7
	Panel b: Log(employment)			
	All	<=HS	SC	BA
log(MW)	-0.080	-0.072	-0.091	-0.018
	(0.109)	(0.113)	(0.110)	(0.117)
N	25594	25594	25594	25594
DV mean (level)	2417.5	1072.4	758.6	586.5

Notes: Table shows results from the Quarterly Workforce Indicators data, covering years 2000-2017. Nursing staff in nursing homes are identified by women employed in NAICS sector 6231 working in counties that straddle a minimum wage discontinuity. Workers with $\leq$ HS are defined as all employees minus those with at least some college education; SC is defined as those with some college education (education category 3); BA is defined as those with at least a four-year degree (education category 4). $log(MW)$ is defined as the natural log of the highest minimum wage in county c at time t in 2017 dollars, adjusted for inflation using the CPI-U-RS. $log(earnings)$ is the real log average quarterly earnings among workers who were employed at the end of the quarter and $log(employment)$ is the log number of employees in county c for each education group in a county-quarter-education cell who were employed at the end of the quarter. All specifications include county-pair-quarter and county fixed effects and controls for county employment rates and the elderly population share; and state EITC parameters, the share of the elderly population receiving Supplemental Security Income, and AFDC/TANF caseloads and benefit levels. All cells are weighted by county population. Robust standard errors clustered by county. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table 1.4: Alternative Measures of Nursing Home Earnings

	(1)	(2)	(3)	(4)
	OSHPD	Current Population Survey		American Community Survey
	Log(annual wage bill)	Log(hourly wage)	Log(wkly earnings)	Log(annual salary)
		Panel a: Nursing assistants		
log(MW)	0.142***	0.115**	0.199**	0.339**
	(0.016)	(0.045)	(0.077)	(0.139)
N	45324	23556	23556	51234
DV mean (level)	29361.99	12.05	448.90	20117.60
MW mean	9.10	7.60	7.60	7.62
		Panel b: LPN/LVNs		
log(MW)	-0.0609	-0.151*	-0.319**	0.270
	(0.078)	(0.086)	(0.139)	(0.193)
N	45303	4969	4969	17675
DV mean	59356.87	19.74	747.90	35244.80
MW mean	9.07	7.62	7.62	7.58

Table 1.4: (continued)

	(1)	(2)	(3)	(4)
	OSHPD	Current Population Survey		American Community Survey
	Log(annual wage bill)	Log(hourly wage)	Log(wkly earnings)	Log(annual salary)
Panel b: RNs				
log(MW)	0.0817	-0.146	-0.130	-0.270
	(0.123)	(0.142)	(0.137)	(0.201)
N	44782	6122	6122	15712
DV mean	78147.42	25.43	962.00	48211.40
MW mean	9.09	7.69	7.69	7.68
Geo FE	Facility	State	State	PUMA
Geo X year FE	Cty pair	Division	Division	Division
Area business cycle controls	X	X	X	X
Demographic controls	X	X		X
State linear trends		X	X	X

Notes: Table shows average wages for nursing home workers by occupation using earnings measures from the California OSHPD (column (1), 2003-2017), the CPS Outgoing Rotation Groups (columns (2-3), 1991-2017), and decennial Census and ACS (column (4), 2000-2017). $log(MW)$ is the natural log of the local or state minimum wage (column (1)), county (for those living in identifiable urban areas) or state minimum (columns (2-3)) or maximum minimum wage in a PUMA (column (4)) at time t in 2017 dollars, adjusted for inflation using the CPI-U-RS. All specifications include controls for local employment rates, share of population older than 65, and facility demographic characteristics. Column (1) includes county-pair by year fixed effects; columns (2-4) include state linear trends and Census division-by-year fixed effects. Column (1) additionally includes facility fixed effects; columns (2-3) include state fixed effects; column (4) includes PUMA fixed effects. Column (1) is weighted by the number of beds in a facility; specifications in columns (2-4) use person weights for the respective survey. Standard errors clustered by county (column (1)), state (columns (2-3)) or PUMA (column (4)). See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table 1.5: Minimum Wages and Employment: Facility Administrative Data

	(1)	(2)	(3)	(4)	(5)	(6)
	Nursing assistant		Vocational nurse		Registered nurse	
Panel a: Log hours per resident day						
log(MW)	0.0049	0.0048	0.0292	0.0236	-0.0955**	-0.1025**
	(0.0187)	(0.0186)	(0.0236)	(0.0236)	(0.0412)	(0.0408)
Observations	269059	269059	266653	266653	267922	267922
DV mean (level)	2.271	2.271	0.786	0.786	0.484	0.484
Panel b: Log number total employees						
log(MW)	0.0628***	0.0643***	0.0258	0.0253	-0.0289	-0.0388
	(0.0239)	(0.0239)	(0.0274)	(0.0276)	(0.0341)	(0.0346)
Observations	404777	404777	403367	403367	408058	408058
DV mean (level)	38.079	38.079	12.815	12.815	7.458	7.458
Demographic controls		X		X		X

Notes: Table shows staffing results from the OSCAR/CASPER staffing reports reported by facilities to CMS, covering years 2000-2016 (panel a) and 1992-2017 (panel b). Sample includes facilities in counties that straddle a minimum wage discontinuity. $log(MW)$ is defined as the natural log of the county minimum wage at time t in 2017 dollars, adjusted for inflation using the CPI-U-RS. Log hours per resident day is defined as the number of staffing hours for each occupation divided by the number of patients times 24 (including direct care and administrative time). Log number total employees is defined as the (natural log) of full-time equivalent workers for each occupation group. All specifications include county-pair-time and facility fixed effects and controls for county employment rates and the elderly population share; and state EITC parameters, the share of the elderly population receiving Supplemental Security Income, and AFDC/TANF caseloads and benefit levels. Odd-numbered columns do not include controls for facility patient mix; even-numbered columns control for facility market concentration and resident demographic characteristics at the facility level: average resident age, and the share of residents female, white, black, and covered by Medicaid. Robust standard errors clustered by county. All regressions weighted by facility size. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table 1.6: Minimum Wages and Worker Flows: Quarterly Workforce Indicators

	(1)	(2)	(3)	(4)
	Panel a: Log(turnover)			
	All	<=HS	SC	BA
log(MW)	-0.055	-0.053	-0.044	-0.096
	(0.103)	(0.100)	(0.108)	(0.116)
N	10164	10164	10164	10164
DV mean (rate)	0.171	0.199	0.150	0.145
	Panel b: Log(hires, employed 1+ qtr)			
	All	<=HS	SC	BA
log(MW)	0.271	0.359**	0.261	0.193
	(0.175)	(0.174)	(0.183)	(0.203)
N	9208	9208	9208	9208
DV mean (rate)	0.096	0.111	0.083	0.084
	Panel c: Log(separations)			
	All	<=HS	SC	BA
log(MW)	-0.290**	-0.238*	-0.304**	-0.377***
	(0.131)	(0.140)	(0.130)	(0.144)
N	14380	14380	14380	14380
DV mean (rate)	0.172	0.196	0.153	0.148

Notes: Table shows results from the Quarterly Workforce Indicators data, covering years 2000-2017. Nursing staff in nursing homes are identified by women employed in NAICS sector 6231 working in counties that straddle a minimum wage discontinuity. Workers with $\leq$ HS are defined as all employees minus those with at least some college education; SC is defined as those with some college education (education category 3); BA is defined as those with at least a four-year degree (education category 4). $log(MW)$ is defined as the natural log of the highest minimum wage in county c at time t in 2017 dollars, adjusted for inflation using the CPI-U-RS. $log(turnover)$ is the log sum of all hires and all separations, divided by two; $log(hires, employed1{+}qtr)$ is the natural log of the hires who remained employed for at least three months; and $log(separations)$ is the natural log of the the number of workers who separated from their employer in a county-quarter-education cell. All specifications include county-pair-quarter and county fixed effects and controls for county employment rates and the elderly population share; and state EITC parameters, the share of the elderly population receiving Supplemental Security Income, and AFDC/TANF caseloads and benefit levels. All cells are weighted by county population. Robust standard errors clustered by county. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table 1.7: Minimum Wages and Worker Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Citizen	White	Female	$\leq$ HS	Prime age	Married	Parent
Panel a: Current Population Survey							
log(MW)	-0.0522	-0.0448	-0.0340	0.0002	0.0827	0.0093	-0.0439
	(0.0468)	(0.0777)	(0.0753)	(0.0947)	(0.0819)	(0.0904)	(0.0834)
N	24883	24883	24883	24883	24883	24883	24883
DV mean	0.803	0.505	0.915	0.638	0.691	0.410	0.564
ϵ_{mw}	-0.0650	-0.0887	-0.0372	0.0003	0.1197	0.0227	-0.0778
Geo FE	State	State	State	State	State	State	State
Panel b: American Community Survey							
log(MW)	0.0136	-0.002	0.0171	-0.112	0.006	0.0610	-0.0662
	(0.0340)	(0.0739)	(0.0559)	(0.101)	(0.100)	(0.0991)	(0.102)
N	52590	52590	52590	52590	52590	52590	52590
DV mean	0.979	0.693	0.927	0.684	0.643	0.385	0.528
ϵ_{mw}	0.0139	-0.0023	0.0184	-0.1637	0.0086	0.1584	-0.1254
Geo FE	PUMA	PUMA	PUMA	PUMA	PUMA	PUMA	PUMA
Area business cycle controls	X	X	X	X	X	X	X
State linear trends	X	X	X	X	X	X	X
Division X year FE	X	X	X	X	X	X	X

Notes: Table shows average demographic characteristics for nursing assistants employed in nursing home settings from the Current Population Survey Outgoing Rotation Groups (panel (a), covering years 1991 through 2017), and decennial Census and American Community Survey (panel (b), covering years 2000 through 2017). $log(MW)$ is defined as the natural log of the local or state minimum wage (panel (a)), county (for those living in identifiable urban areas) or state minimum (columns (2) and (3)) or maximum minimum wage in a PUMA (panel (b)) at time t in 2017 dollars, adjusted for inflation using the CPI-U-RS. All specifications include state linear trends; Census division-by-year fixed effects; and controls for local county employment rates and the elderly population share, state EITC parameters, the share of the state elderly population receiving Supplemental Security Income, and state AFDC/TANF caseloads and benefit levels. Panel (a) additionally includes state fixed effects and panel (b) includes Public-Use Microdata Area (PUMA) fixed effects. Standard errors clustered by state (panel (a)) or PUMA (panel (b)). See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table 1.8: Minimum Wages and Health Inspection Violations

	(1)	(2)	(3)	(4)	(5)
Panel a: All health violations					
	Any	Number	Any severe	Number severe	Standardized score
log(MW)	-0.0004	-0.6149*	0.0711***	0.0911	-0.1164*
	(0.0097)	(0.3437)	(0.0246)	(0.0610)	(0.0651)
N	345102	345102	345102	345102	345102
DV mean	0.958	6.428	0.1664	0.3309	0.0328
Demographic controls	X	X	X	X	X
ϵ_{mw}	0.000	-0.096	0.427	0.275	
Panel b: Quality of care (QOC) violations					
	Any	Number	Any severe	Number severe	Standardized score
log(MW)	-0.0540**	-0.7408***	0.0336	0.0562	-0.0750
	(0.0243)	(0.1985)	(0.0222)	(0.0367)	(0.0535)
N	345102	345102	345102	345102	345102
DV mean	0.8677	3.5282	0.1360	0.1869	-0.0146
Demographic controls	X	X	X	X	X
ϵ_{mw}	-0.062	-0.210	0.247	0.301	

Notes: Table shows results from the state health inspection reports reported to CMS, covering years 1998-2017. Sample includes facilities in counties that straddle a minimum wage discontinuity. $log(MW)$ is defined as the natural log of the minimum wage faced by facility f at time t in 2017 dollars, adjusted for inflation using the CPI-U-RS. "Severe" violations are those presenting actual harm or immediate jeopardy to residents (CMS categories G-L). "Quality of care" violations follow the definition in Harrington et al. (2001) to include violations in the quality of care, assessment, nursing, dietary, physician, rehabilitative services, dental, and pharmacy regulation categories. "Standardized score" allocates violation points to each violation based on the CMS scoring criteria and normalizes the score distribution across facilities as in Kling et al. (2007) and Anderson (2008) (Centers for Medicare and Medicaid Services, 2011). All specifications include county-pair-time and facility fixed effects and controls for county employment rates and the elderly population share; and state EITC parameters, the share of the elderly population receiving Supplemental Security Income, and AFDC/TANF caseloads and benefit levels; and facility average resident age, market concentration, and the share of residents female, white, black, and covered by Medicaid. Robust standard errors clustered by county. All regressions weighted by facility size. "ϵ_{mw}" summarizes the elasticity of the outcome measure with respect to the minimum wage. See text for details. *** $= p < 0.01$, ** $= p < 0.05$, * $= p < 0.10$.

Table 1.9: Minimum Wages and Patient Health

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Pressure ulcers (share)		UTI (share)		Restraint (share)		Psychotropic (share)		Health index	
log(MW)	-0.0140***	-0.0139***	-0.0065	-0.0075	-0.0079	-0.0081*	0.0318	0.0347	-0.1976***	-0.2063***
	(0.0051)	(0.0050)	(0.0051)	(0.0050)	(0.0048)	(0.0048)	(0.0284)	(0.0283)	(0.0727)	(0.0717)
N	289855	289855	329915	329915	330077	330077	179169	179169	286092	286092
DV mean	0.083	0.083	0.073	0.073	0.025	0.025	0.190	0.190	-0.104	-0.104
Demographic controls		X		X		X		X		X
Δ # residents (1000s), 10% increase	-1.89	-1.87	-0.88	-1.01	-1.06	-1.09	4.29	4.68		
ϵ_{mw}	-0.1681	-0.1669	-0.0895	-0.1033	-0.3173	-0.3253	0.1674	0.1826		

Notes: Table shows patient outcomes results from long-term resident assessment reports reported by facilities to CMS, covering years 2005-2017. Sample includes facilities in counties that straddle a minimum wage discontinuity. $log(MW)$ is defined as the natural log of the minimum wage faced by facility f at time t in 2017 dollars, adjusted for inflation using the CPI-U-RS. All variables are winsorized at the 99th percentile to exclude extreme values. All specifications include county-pair-time and facility fixed effects and controls for county employment rates and the elderly population share; and state EITC parameters, the share of the elderly population receiving Supplemental Security Income, and AFDC/TANF caseloads and benefit levels. Even-numbered columns also include controls for average resident age, facility market concentration, and the share of residents female, white, black, and covered by Medicaid. Robust standard errors clustered by county. All regressions weighted by facility size. "ϵ_{mw}" summarizes the elasticity of the outcome measure with respect to the minimum wage. "Δ # residents (1000s), 10% increase" summarizes the estimated change in the number of residents for a 10 percent increase in the minimum wage. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table 1.10: Minimum Wages and Log Elderly Mortality Rates, by Place of Death

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All		Nursing homes		Non-nursing homes		Hospitals	
log(MW)	-0.0722**	-0.0509*	-0.333***	-0.311***	-0.0068	0.0145	0.0327	0.0574
	(0.0330)	(0.0289)	(0.118)	(0.114)	(0.0424)	(0.0397)	(0.0839)	(0.0824)
N	55306	55306	50680	50680	55258	55258	40038	40038
DV mean (level)	0.0516	0.0516	0.0158	0.0158	0.0365	0.0365	0.0228	0.0228
Δ # residents (1000s), 10% increase	n/a	n/a	-16.238	-15.165	n/a	n/a	n/a	n/a
County controls	X	X	X	X	X	X	X	X
Demographic controls		X		X		X		X

Notes: Table shows county-level age-adjusted log mortality rates covering years 1990-2013 for the population ages 65 and older by place of death. The age adjustment, defined in Equation 1.4, holds the age composition of the population fixed at its 2000 distribution; see Stevens et al. (2015) for technical details. Sample includes counties that straddle a minimum wage discontinuity. $log(MW)$ is defined as the natural log of the highest minimum wage in county c at time t in 2017 dollars, adjusted for inflation using the CPI-U-RS. All specifications include county-pair-year and county fixed effects and controls for county employment rates and the elderly population share; and state EITC parameters, the share of the elderly population receiving Supplemental Security Income, and AFDC/TANF caseloads and benefit levels. Demographic controls include CZ-level market concentration and county-average resident age, and the share of residents female, white, black, and covered by Medicaid. Robust standard errors clustered by county. All regressions weighted by county elderly population. "Δ # residents (1000s), 10% increase" summarizes the estimated change in the number of residents for a 10 percent increase in the minimum wage. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table 1.11: Minimum Wages, Payment Methods, and Care Needs

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Resident share			Average resident care needs		Log avg Medicare rate		Discharge	Hospital	Occupancy
	Medicaid	Other	Medicare	ADL index	Care index	1996-2010	2011-2017	rate	admit rate	rate
log(MW)	-0.0264**	0.0249**	0.0082	0.1811***	0.2138***	-0.0079	-0.0199	-0.0129	-0.0134**	0.0076
	(0.0129)	(0.0119)	(0.0060)	(0.0382)	(0.0335)	(0.0166)	(0.0154)	(0.0101)	(0.0053)	(0.0064)
N	729778	729623	729986	714858	722459	144915	96552	287251	328676	729510
DV mean	0.5927	0.2362	0.1517	0.0086	0.0033	333.8	458.0	0.6202	0.1785	0.8439
ϵ_{mw}	-0.0445	0.1054	0.0541					-0.0208	-0.0751	0.0090

Notes: Table the share of nursing home residents by payment source (columns (1) through (3)); average standardized care needs (columns (4) and (5)); and discharge, transfer, and occupancy rate (columns (8) through (10)) derived from resident assessment reports reported by facilities to CMS covering years 2000 through 2016, as well as the average reimbursement rate for among residents covered by Medicare based on RUG classification (columns (6) and (7)) from Medicare cost reports (HCRIS) reported to CMS, covering years 1996-2017. Sample includes facilities in counties that straddle a minimum wage discontinuity. $log(MW)$ is defined as the natural log of the minimum wage faced by facility f at time t in 2017 dollars, adjusted for inflation using the CPI-U-RS. All specifications include county-pair-year and facility fixed effects and controls for county employment rates and the elderly population share; and state EITC parameters, the share of the elderly population receiving Supplemental Security Income, and AFDC/TANF caseloads and benefit levels. Columns (6) and (7) additionally limit the sample to facilities with a reporting period that starts and ends after February of the ending year and includes controls the starting and ending months of the cost reporting period; the number of beds in the facility; the days in the reporting period; and whether a reporting period was less than 11 or more than 13 months. Robust standard errors clustered by county. All regressions weighted by facility size. "ϵ_{mw}" summarizes the elasticity of the outcome measure with respect to the minimum wage. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table 1.12: Minimum Wages and Patient Demographic Characteristics

	(1)	(2)	(3)	(4)
	Avg age	Share female	Share black	Share white
log(MW)	-0.2471	-0.0215***	-0.0088	0.0042
	(0.1853)	(0.0043)	(0.0065)	(0.0057)
N	700443	687142	405164	680773
DV mean	80.09	0.6856	0.1690	0.7965
ϵ_{mw}	-0.0031	-0.0314	-0.0521	0.0053

Notes: Table shows patient characteristics derived from resident assessment reports reported by facilities to CMS, covering years 2000-2017. Sample includes facilities in counties that straddle a minimum wage discontinuity. $log(MW)$ is defined as the natural log of the minimum wage faced by facility f at time t in 2017 dollars, adjusted for inflation using the CPI-U-RS. All specifications include county-pair-time and facility fixed effects and controls for county employment rates and the elderly population share; and state EITC parameters, the share of the elderly population receiving Supplemental Security Income, and AFDC/TANF caseloads and benefit levels. Robust standard errors clustered by county. All regressions weighted by facility size. "ϵ_{mw}" summarizes the elasticity of the outcome measure with respect to the minimum wage. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table 1.13: Patient Safety and Health, by Provider Characteristics

	(1)	(2)	(3)	(4)	(5)
	High Medicaid share	Private ownership	Multi-establishment chain	Competitive industry	Direct care staff req't
			Panel a: Number of QOC violations		
log(MW)	-0.751**	-0.834**	-0.776**	-0.904**	-0.751**
	(0.341)	(0.341)	(0.343)	(0.362)	(0.344)
log(MW) X char	-0.009	0.094**	0.019	0.160	-0.004
	(0.029)	(0.038)	(0.027)	(0.107)	(0.045)
N	114722	114722	114030	114722	114722
DV mean | char = 0	3.535	3.211	3.517	3.718	3.452
DV mean | char = 1	3.719	3.796	3.748	3.628	3.710
E(char) = 1	0.535	0.723	0.539	0.941	0.704
			Panel b: Share with pressure ulcers		
log(MW)	-0.018**	-0.018***	-0.018***	-0.022***	-0.016***
	(0.0080)	(0.006)	(0.006)	(0.006)	(0.006)
log(MW) X char	0.001	0.000	-0.001	0.004**	-0.004**
	(0.011)	(0.001)	(0.001)	(0.002)	(0.002)
N	262024	262024	261277	262024	262024
DV mean | char = 0	0.085	0.081	0.092	0.076	0.090
DV mean | char = 1	0.095	0.094	0.089	0.091	0.091
E(char) = 1	0.565	0.713	0.511	0.953	0.760

Table 1.13: (continued)

	(1)	(2)	(3)	(4)	(5)
	High Medicaid share	Private ownership	Multi-establishment chain	Competitive industry	Direct care staff req't
	Panel c: Nursing home log mortality rate				
log(MW)	-0.157	-0.122	-0.099	-0.112	-0.103
	(0.111)	(0.108)	(0.107)	(0.109)	(0.110)
log(MW) X char	0.114	0.000	-0.027***	-0.011	-0.018
	(0.142)	(0.014)	(0.011)	(0.015)	(0.012)
N	31320	31328	31294	31328	31328
DV mean | char $<$ 0.5	0.013	0.014	0.012	0.013	0.015
DV mean | char $\geq$ 0.5	0.012	0.013	0.014	0.013	0.012
E(char) = 1	0.172	0.736	0.554	0.969	0.688

Notes: Table shows patient outcomes results disaggregated by provider characteristics. Sample includes facilities in counties (panels (a) and (b)) and counties (panel (c)) that straddle a minimum wage discontinuity. $log(MW)$ is defined as the natural log of the minimum wage faced at time t in 2017 dollars, adjusted for inflation using the CPI-U-RS. *char* is an indicator equal to one for facilities satisfying each characteristic in the column header (high Medicaid share, private ownership, chain, located in a competitive industry, or in a state with a minimum staffing requirement for direct care staff) in panels (a) and (b), and the share of nursing home beds satisfying each characteristic in panel (c). All specifications include county-pair-time fixed effects and controls for county employment rates and the elderly population share; state EITC parameters, the share of the elderly population receiving Supplemental Security Income, and AFDC/TANF caseloads and benefit levels; and facility average resident age, facility market concentration, and the share of residents female, white, black, and covered by Medicaid. Panels (a) and (b) additionally include facility fixed effects; panel (c) includes county fixed effects. Robust standard errors clustered by county. Regressions weighted by facility size (panels (a) and (b)) or size of the elderly population (panel (c)). See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

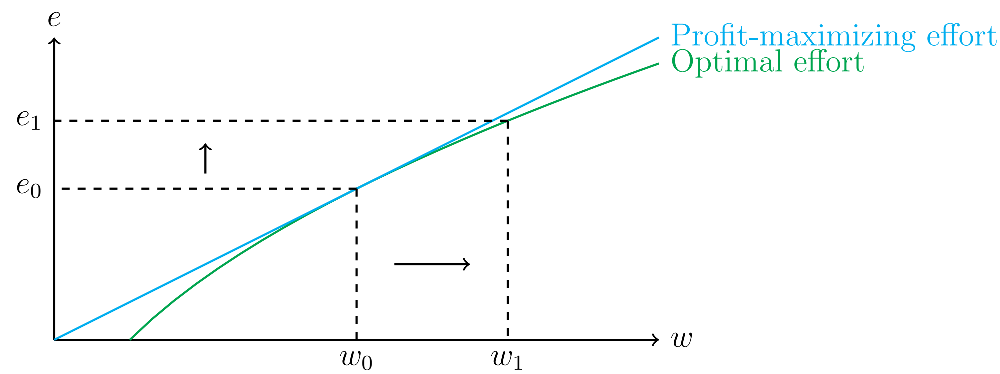
Table 1.14: Minimum Wages and Facility Revenue and Costs

	(1)	(2)	(3)	(4)	(5)
	Costs/ resident	Charge/ resident	Revenue/ resident	Net income	Avg Mcaid per diem (state)
log(MW)	0.0434*	0.0874	0.0687*	-1.4430	-0.0269
	(0.0244)	(0.1020)	(0.0352)	(1.1480)	(0.0853)
N	286988	279082	273940	287723	480
DV mean (level)	75108.4	16997.0	91717.2	1440.5	184.57
County controls	X	X	X	X	X
# days	X	X	X	X	
Log(beds)	X	X	X	X	
DV transformation	Log	Log	Log	IHS	Log

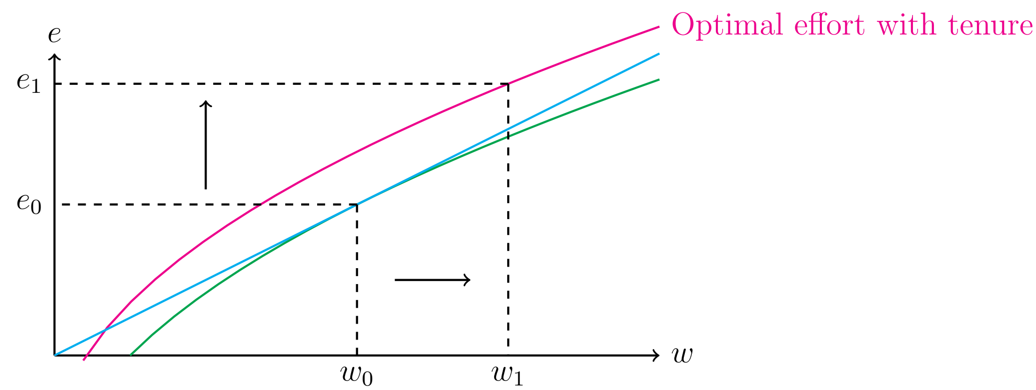
Notes: Table shows facility revenues and cost metrics from Medicare cost reports (HCRIS) reported to CMS, covering years 1996-2017 (columns (1-4)) and average state Medicaid reimursement rates from Brown School of Public Health (2019) (column 5). Sample in columns (1-4) includes facilities in counties that straddle a minimum wage discontinuity, with a reporting period that starts and ends in January or February of the ending year. $log(MW)$ is defined as the natural log of the minimum wage faced by facility f at time t in 2017 dollars, adjusted for inflation using the CPI-U-RS. All specifications include county-pair-year and facility fixed effects and controls for county employment rates and the elderly population share; state EITC parameters, the share of the elderly population receiving Supplemental Security Income, and AFDC/TANF caseloads and benefit levels; the starting and ending months of the cost reporting period; the number of beds in the facility; the days in the reporting period; and whether a reporting period was less than 11 or more than 13 months. Dependent variable is the log of the cost, charge, or revenues per resident (columns (1-3), respectively), or the inverse hyperbolic sine of net income (column (4)). Robust standard errors clustered by county. All regressions weighted by facility size. Column (5) estimates the two-way state and year fixed effect model in Equation 1.3 with division-by-year fixed effects and state linear trends at the state level (standard errors clustered by state). See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Figure 1.1: Worker Effort Incentives

(a) No change in tenure

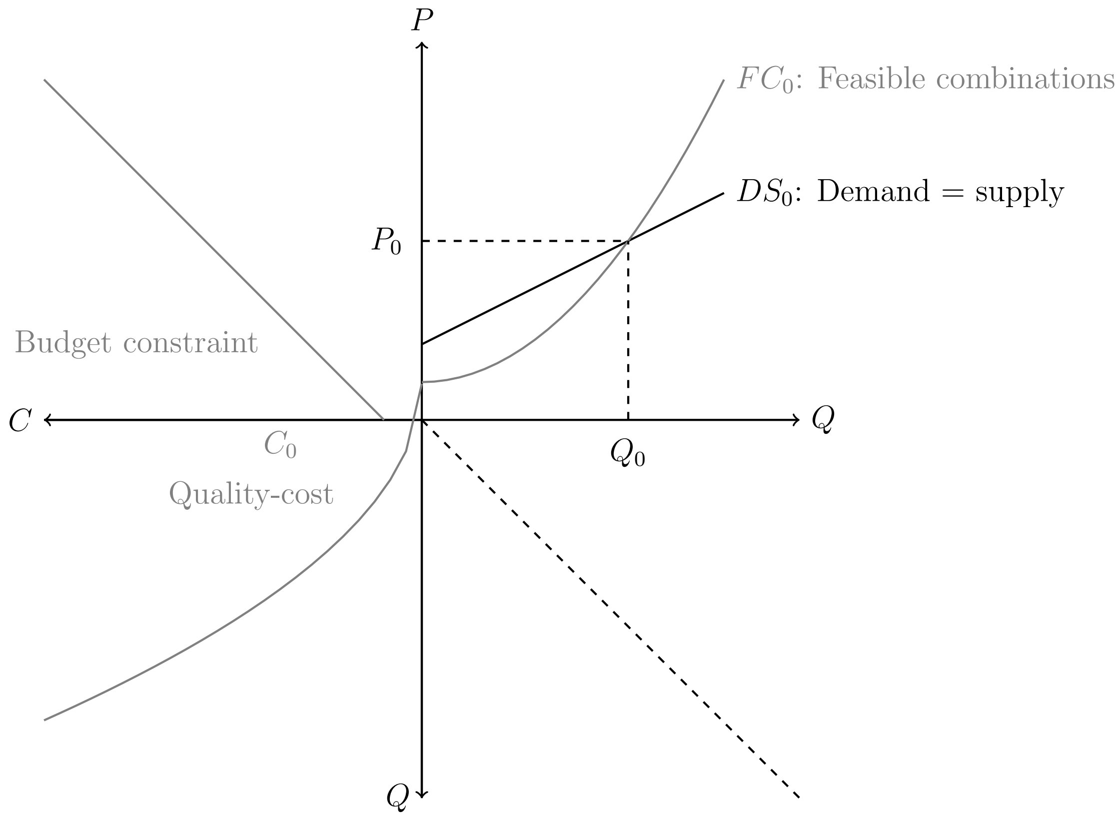


(b) Increase in tenure



Notes: Figure shows a possible relationship between wages w and the effort level e that maximizes utility (green). See Section 1.3 for a heuristic description and Appendix A.2 for a more technical derivation. The linear blue line shows the cost-minimizing level of effort for the firm. Each actor assumes the other will act rationally and equilibrium is given by (w_0, e_0). Panel (a) shows that if human capital does not change, a binding minimum wages moves actors away from this equilibrium to (w_1, e_1) – effort levels increase, but less than the proportional increase in firm costs. Panel (b) shows that if minimum wages lead to greater worker retention, workers obtain greater human capital, reducing the costs associated with any effort level. Increased tenure amplifies any efficiency wage channel and leads to even higher service quality.

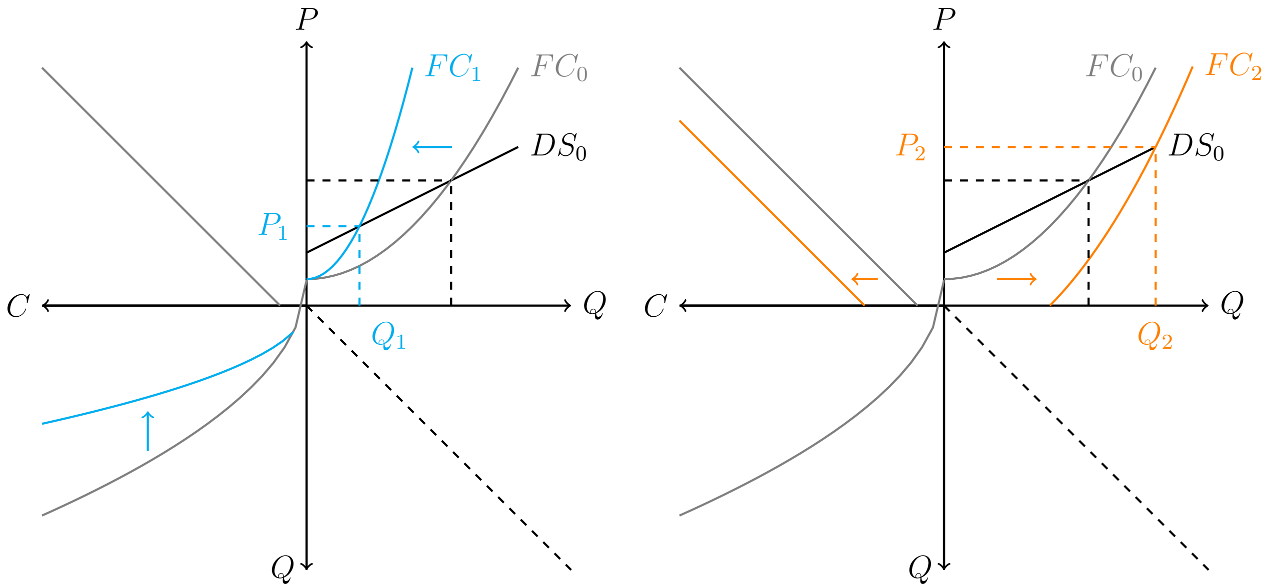
Figure 1.2: Market Equilibrium, No Minimum Wages



Notes: Figure shows a possible market equilibrium based on price, quality, and cost decisions. DS maps all the price-quality combinations that equate nursing home supply with demand, and FC indicates all price-quality combinations that are feasible with the production technology in Quadrant III and satisfy the firm's revenue constrain in Quadrant IV. See Section 1.3 for greater detail.

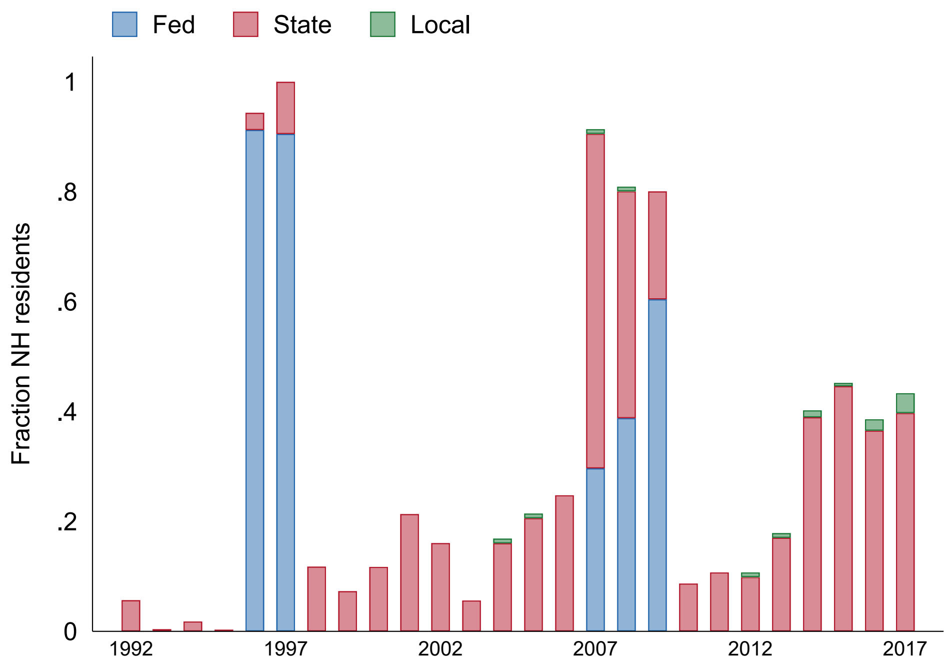
Figure 1.3: Market Equilibrium, Minimum Wage Adjustments

(a) Minimum wages reduce production efficiency (b) Minimum wages lower firm profits



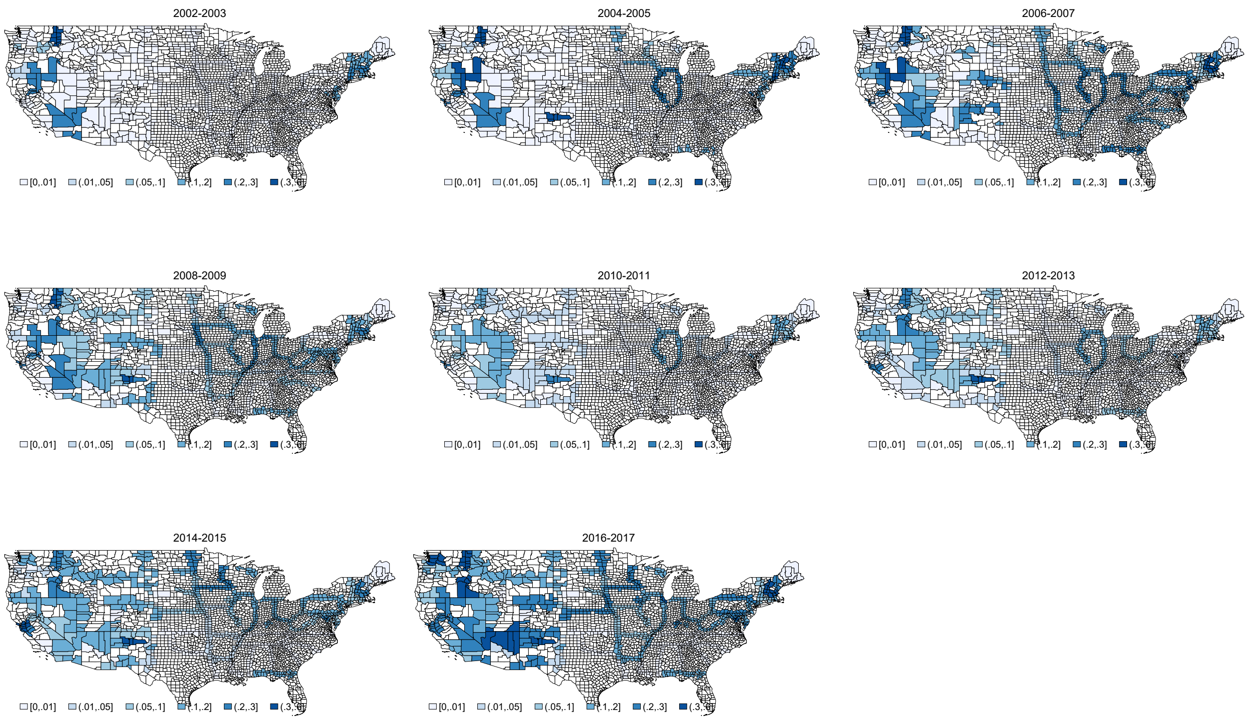
Notes: Figure shows two possible changes in market equilibrium following a statutory increase in the minimum wage. DS maps all the price-quality combinations that equate nursing home supply with demand, and FC indicates all price-quality combinations that are feasible with the production technology in Quadrant III and satisfy the firm's revenue constrain in Quadrant IV. See Section 1.3 for greater detail. Panel (a) presents the hypothetical situation where higher minimum wages increase firm costs and lead to an inefficient factor mix, reducing quality and price. Panel (b) presents the hypothetical situation where higher minimum wages reduce firm profits, potentially increasing prices and quality.

Figure 1.4: Number of Nursing Home Residents in Jurisdiction with Minimum Wage Increase, County Pairs Sample



Notes: Figure shows the share of the nursing home residents in the county pairs sample living in a jurisdiction that experienced a minimum wage reform in relative to the previous year by the level at which the reform occurred (federal, state, or substate).

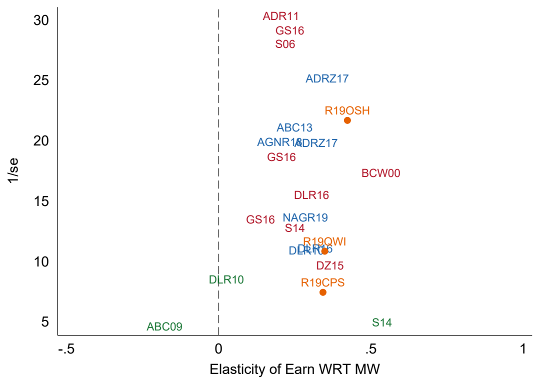
Figure 1.5: County Pair Log Minimum Wage Differential, by Year



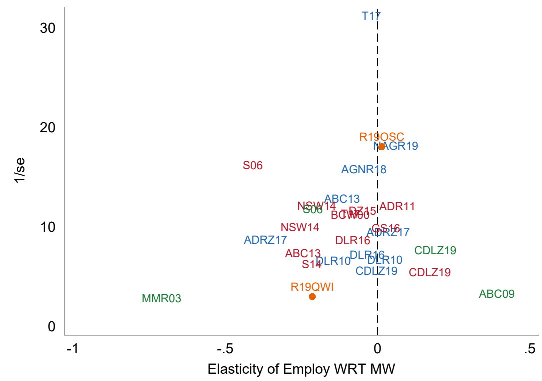
Notes: Figure shows the maximum difference in (inflation-adjusted) log minimum wages between adjacent counties for each two year period. See text for details.

Figure 1.6: Employment and Earnings Responses, Nursing Home Workers and Previous Literature

(a) Earnings

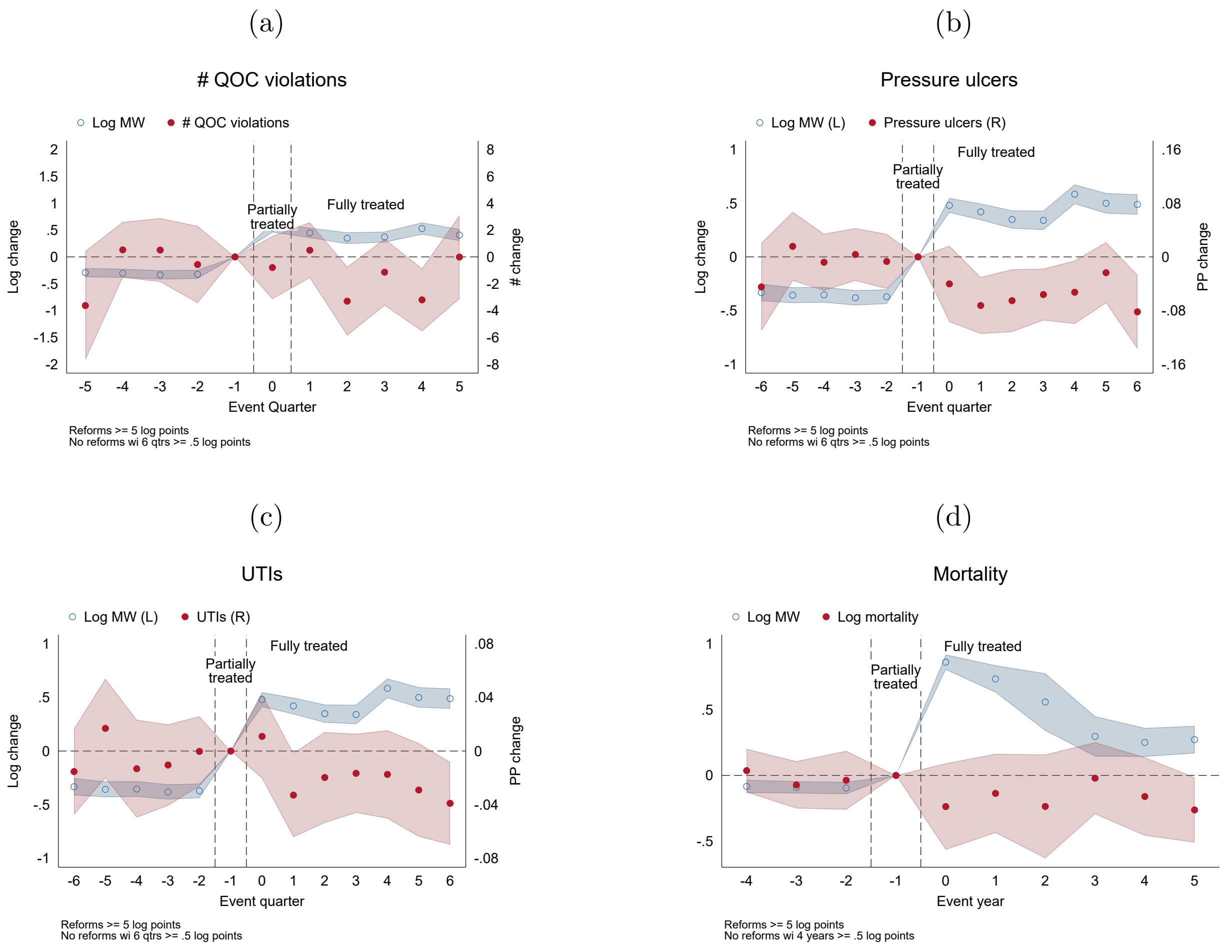


(b) Employment



Notes: Figure shows a funnel plot estimates of the elasticity of earnings (panel (a)) and employment (panel (b)) with respect to the minimum wage, scaled by the fraction of low-wage workers. The fraction of low-wage workers is estimated as the share of workers of each demographic group or occupation earning within 127 percent (110 percent times 115 percent) of the current minimum wage from the 2014-2018 CPS-ORG. Labels denote author initials and year; see reference list for full citations. Blue labels show estimates for restaurant workers, maroon for teenagers, orange for US nursing home workers, and green for all other groups.

Figure 1.7: Event Studies, Patient Outcomes and Minimum Wages



Notes: Figure shows event studies from Equation 1.5. Blue line indicates the change in the minimum wage; red line shows the change in patient outcomes: the number of quality of care violations (panel (a)) pressure ulcers (panel (b)), UTIs (panel (c)), and log mortality (panel (d)). Sample is limited to reforms that changed the within-county-pair log gap by at least 5 log points and for which there were no changes greater than 0.5 log points in the preceeding six quarters (panels (a) through (c)) or four years (panel (d)). All specifications include controls for county employment rates and the elderly population share; state EITC parameters, the share of the elderly population receiving Supplemental Security Income, and AFDC/TANF caseloads and benefit levels; and county-pair-year and reform year fixed effects. Panels (a) through (c) additionally include facility and quarter fixed effects; panel (d) includes county fixed effects. Shaded areas indicate 95 percent confidence intervals with robust standard errors clustered at the county level. P-value of test all pre-reform coefficients for patient outcomes equal zero is: 0.942 (panel (a)); 0.870 (panel (b)); 0.667 (panel (c)); and 0.936 (panel (d)). See text for details.

Chapter 2

Universal Access to Free School Meals and Student Achievement

2.1 Introduction

The National School Lunch Program (NSLP) and School Breakfast Program (SBP) – jointly referred to as the school meals program – are the largest nutritional assistance programs serving school-aged children. On a typical school day, more than 30 percent of 5-17 year-olds receive free lunches, and more than 20 percent receive free breakfasts (USDA, 2018b).[1] These programs represent a large share of students' nutritional intake, as children consume up to half of their daily calories at school (Gleason and Suitor, 2001). School meals also provide a relatively generous income subsidy to low-income families: a student receiving free breakfasts and lunches pays approximately $4.50 a day ($800 per school year) less than students paying the full price.[2]

Despite the size and importance of the school meals program, evaluating the causal effect of school-based nutritional assistance on child outcomes presents empirical challenges. Until recently, there was little program variation across schools or over time: most schools implemented the lunch component within a short time period, the federal government establishes requirements for all schools, and nearly all schools participate. At the student level, family income determines payment rates. Accordingly, children who receive free meals are systematically more disadvantaged than ineligible students, which complicates comparisons of eligible and ineligible children. While the existing literature finds school meals increase food consumption and nutritional intake (Bhattacharya et al., 2006; Schanzenbach, 2009; Gleason and Suitor, 2001; Nord and Romig, 2006; Gundersen et al., 2012), results for other outcomes

[1] An additional five percent receive reduced-price meals at a deeply subsidized rate.

[2] In comparison, the average daily per-person SNAP benefit for families with children is about $4.00 (USDA, 2018a). The size of the school meals program is also apparent by examining federal outlays. In Fiscal Year 2017, the federal government allocated about $16.8 billion to school nutrition programs, compared to $15.0 billion in Title I funding and $28.0 billion in pro-rated SNAP benefits to children (USDA, 2017; US Department of Education, 2017; USDA, 2018a)

are more mixed (Dunifon and Kowaleski-Jones, 2003; Frisvold, 2015; Hinrichs, 2010; Meyers et al., 1989).

Recent reforms have transformed the school meals program from income-based assistance to more universal access by allowing schools and districts to offer free meals to all students, regardless of a student's family income. The shift towards school-based assistance has fundamentally altered the nature of the program: in the 2019 school year, more than a quarter of school-aged children attended a school offering universal meals, a marked increase from less than one percent in 2012 (Food Research and Action Center, 2019). This chapter examines the most recent and largest such reform, the Community Eligibility Provision (CEP). Although universal programs formally increase access to free school meals, schools and districts with relatively high free meal participation under the traditional program (those that were *de facto* approaching universal provision) have the greatest incentives to participate. As CEP participation is voluntary, *ex-ante* it is unclear whether moving to universal assistance will affect meal participation or student test scores in these schools and districts.

In order to examine the effect of schoolwide free meals on consumption and student performance, I compare changes in early-adopting districts to those adopting later. This differences-in-differences approach relies on the fact that although CEP is a federal program, not all schools and districts became eligible for or adopted schoolwide free meals at the same time. Districts became eligible for CEP over a four-year period depending on state, and within states, participation among eligible schools and districts has increased over time. Importantly, this framework accounts for selection into CEP by limiting the analysis sample to "ever-adopting" districts – those with similar observed and unobserved participation incentives. If the timing of CEP adoption is uncorrelated with changes in potential performance, this approach identifies the causal effect of universal access to free meals.

To evaluate whether the timing of CEP adoption is plausibly exogenous among these ever-adopting districts, I conduct two complementary analyses. First, I explore whether baseline characteristics, such as district resources and economic well-being, systematically differ between early- and late-adopting districts. Here, I find that districts adopting in the first pilot year have slightly higher poverty rates and worse academic performance than areas adopted later, but economic conditions are not differently *trending* for the earliest adopters. Second, I present event study analyses illustrating trends in academic performance before and after CEP adoption. These plots show math performance was not systematically trending for black and Hispanic students prior to CEP adoption. On the other hand, this analysis suggests math performance among white students was improving prior to implementation. Therefore, while the timing of CEP adoption is more likely exogenous from the perspective of non-white students' performance trajectories, the findings for white students should be interpreted cautiously.

My findings are twofold. First, using administrative meal count data from six of the eleven pilot states, I find that even among districts with high baseline free meals eligibility, CEP increased the number of breakfasts and lunches served by approximately 38 and 12 percent, respectively.

Second, I examine how schoolwide free meals affects student performance in all CEP-participating districts nationwide. Improvements in math achievement vary by the share of students gaining access to free meals, with CEP improving performance in districts with the lowest eligibility rates under the traditional program, but not significantly affecting test scores in districts with high baseline free meal eligibility. These patterns are consistent with the nature of the program. Specifically, the full sample of participating districts includes areas with high free meal participation rates under the traditional program and these areas experienced little effective change in access under CEP.

In order to focus on districts that experienced the largest changes in access to free meals, I divide the sample of CEP-participating districts at the median baseline share of students qualifying for free and reduced meals – approximately 58 percent. For the "exposed" districts with relatively low baseline eligibility rates, CEP modestly improved math performance by about 0.02 standard deviations. Scaling these performance improvements by the share of students gaining access to free meals (32 percent) implies that access to free school meals improve math performance by approximate 0.05 standard deviations. Within the exposed subsample, improvements are concentrated among elementary and Hispanic students. The subgroup analyses are consistent with CEP providing benefits to students gaining access to free meals, as Hispanic students had relatively low free meal eligibility rates under the traditional program (Chaparro et al., 2014; Goerge et al., 2009). In contrast to modest improvements in math performance, changes in reading performance are more sensitive to the specification, smaller in magnitude, and generally statistically insignificant.

Math improvements follow an inverse U-shaped pattern over the "exposure" distribution. CEP districts with the lowest baseline eligibility rates tended to adopt CEP in some – but not all – schools, resulting in relatively small increases in free meal access at the district level. On the other end of the distribution, districts with high baseline eligibility rates experienced little change in access, as most students were already eligible for free meals. Districts in the middle of the distribution – those with baseline eligibility rates between approximately 50 and 60 percent – were able to expand free meals to a relatively large share of students. These districts are those that also tended to have the largest improvements in math performance.

There are several channels by which schoolwide free meals may affect student performance. First, students who are otherwise income-eligible, but who did not complete the required paperwork, gain access. Second, higher-income students become eligible free meals. Family resources increase for both of these students, which may yield academic benefits independent of any nutritional changes. Third, if universal access to free meals improves behaviors, it may reduce classroom distractions and increase teaching time, benefiting students whose nutritional consumption does not change. Fourth, since all students receive free meals under CEP, family income may become less salient, which could reduce stigma. Fifth, if CEP participation is determined by financial considerations – either from lower administrative costs or greater federal revenue – districts may provide additional educational supports. These channels are not mutually exclusive, and although the available data do not allow me to fully disentangle among possible mechanisms, results do not meaningfully change after accounting for changes in district resources, indicating the findings are not solely due to concurrent changes in financial or instructional resources.

This chapter builds on a burgeoning literature that uses quasi-experimental variation to estimate the effects of nutritional assistance on health and economic outcomes. Much of the existing research examines family-based assistance through the Supplemental Nutrition Assistance Program (SNAP). For example, Hoynes et al. (2016) find access to SNAP in childhood improves adult outcomes, and Gassman-Pines and Bellows (2015) and Gennetian et al. (2015) find that greater SNAP resources improve short-term student performance and behaviors. The relationship between school-based assistance and children's outcomes is more mixed. While Schanzenbach (2009) finds school meals slightly increase obesity rates, Gleason and Suitor (2001), Schanzenbach and Zaki (2014), and Bhattacharya et al. (2006) find school meals improve nutritional intake. In the long-term, Hinrichs (2010) finds that greater exposure to school lunches increases educational attainment. Examining the short-term effects of these programs can help disentangle whether any long-term benefits arise directly through academic achievement, through latent health benefits, or non-cognitive improvements.

This analysis makes several contributions to the existing literature. First, this chapter examines variation in access to free meals that is related to the characteristics of the surrounding area, but not driven by whether a particular student's family faces economic hardship. Second, the existing work on schoolwide free meal programs is largely limited to the first two years of implementation. As the first districts to adopt CEP are treated for four years in my sample, I am able to explore whether the marginal benefits of nutritional assistance increase or decrease with greater program experience.

In addition, previous work on universal meals almost exclusively focuses on a single, urban school district (Dotter, 2013; Imberman and Kugler, 2014; Schwartz and Rothbart, 2017), or single state (Fuller and Comperatore, 2018; Kho, 2018; Gordanier et al., 2019; Davis and Musaddiq, 2018). This chapter complements the existing research by examining how universal meals affect performance in both rural and urban districts for the near-universe of public school districts, and provides some of the first evidence on the extent to which the experiences of a single state or district reform may apply more broadly. Importantly, the national-level data and staggered adoption period permit a rich set of controls for other state-level changes occurring over the analysis period. To this point, I find improvements in math performance for exposed subgroups are robust to accounting for state-specific trends and leveraging only within-state variation in the timing of CEP adoption. I also find similar effects across geographic regions and states, and between rural and urban areas. In addition, I explore whether the findings are driven by other changes in the school environment by examining how CEP affected district resources and the types of students attending CEP districts and schools. Here, I show total per-pupil expenditures and federal non-nutrition district revenue did not significantly change following implementation. Changes in the student body composition – fewer Hispanic students and greater racial/ethnic segregation – account for no more than 10 percent of the observed improvements.

This chapter proceeds as follows. Section 2.2 reviews the relevant literature. Section 2.3 overviews the CEP reform, and outlines the channels by which school-based assistance can affect student performance. Section 2.4 describes the data and methodology. Section 2.5

presents results and Section 2.6 concludes.

2.2 Existing literature on school meals and universal provision

Food insecurity, defined as inadequate nutritional access, is associated with poor health and impaired social, emotional, and cognitive development (Howard, 2011). A growing body of research finds that nutritional assistance reduces children's food insecurity (Ratcliffe et al., 2011; Mabli and Worthington, 2014; Arteaga and Heflin, 2014; Bhattacharya et al., 2006; Gleason and Suitor, 2001; Frisvold, 2015; Fletcher and Frisvold, 2017; Gundersen et al., 2012). Even accounting for these programs, however, about 16 percent of families with children are food insecure, as household-based assistance often does not cover food costs (Hoynes et al., 2015; Coleman-Jensen et al., 2017) and not all income-eligible students participate in SNAP or the school meals program (Ganong and Liebman, 2013; Domina et al., 2018; Coleman-Jensen et al., 2017). Both prevalence of food insecurity and incomplete take-up suggests there is scope for schoolwide provision to improve children's health and school performance.

The traditional school meals program provides subsidized meals to lower-income school-aged children, with each student's required payment determined by family income: Children with family income below 130 percent of the federal poverty level pay $0 for school breakfasts and lunches, while children in families up to 185 percent of the federal poverty level pay no more than 40 cents. Higher-income students can purchase a meal at the "paid" rate, set by each district and averaging about $3.50 for middle school students. The federal government reimburses schools based on the number of free, reduced, and full-price meals served, with reimbursement rates shown in Table 2.1. Figure 2.1 shows that both the breakfast and lunch programs serve a large share of children, and participation in the free component has grown over time while participation in the paid component has remained relatively steady (breakfast) or declined (lunch).

The existing empirical literature has found mixed results on the effect of the traditional school meals program on student performance. For example, Dunifon and Kowaleski-Jones (2003) find free lunch participation does not significantly change student academic performance, while other work finds slight improvements following greater access to school breakfasts (Meyers et al., 1989; Frisvold, 2015) or more nutritious lunches (Anderson et al., 2017).

Before CEP, schoolwide free meal programs were largely district-initiated efforts that usually only provided free breakfasts. Many of these reforms also changed how meals were provided, for example, by serving breakfast during instructional time, rather than before school (e.g. "Breakfast in the Classroom"). A series of papers examines the effects of these early endeavors and finds universal, in-classroom breakfasts improve math and reading scores (Imberman and Kugler, 2014; Dotter, 2013). On the other hand, schoolwide free breakfast programs that maintain traditional serving methods increase participation, but do not improve performance (Bartlett et al., 2014; Schanzenbach and Zaki, 2014; Leos-Urbel et al., 2013).

With the available data, I am unable to determine whether CEP coincided with changes in how meals were offered. Surveys of school administrators suggest CEP may have affected both access to school meals and how these meals were offered. While most schools continued to operate a traditional "line/cafeteria" service under CEP, about one-third of districts expanded offerings to in-classroom and "grab-and-go" options (Logan et al., 2014). To the extent that CEP changed *how* meals are served, results in this chapter should be interpreted as the "dual" treatment of universal provision and growing likelihood of adopting non-traditional serving methods.

Closely related to this chapter, a number of studies examine the effect of schoolwide free breakfasts and lunches within a single state or district through CEP and other federal initiatives by comparing schools that opt to participate in schoolwide programs to those that do not (either conditional or unconditional on eligibility). Consistent with my results, the existing work tends to find universal free meals modestly improve math performance, particularly for elementary school students, with mixed results on reading and for middle school students (Gordanier et al., 2019; Kho, 2018; Fuller and Comperatore, 2018). Also consistent with benefits being concentrated among populations with low income-based eligibility rates, Schwartz and Rothbart (2017) find schoolwide lunches confer particularly large benefits for students who were income-ineligible ineligible under the traditional program. Finally, using a similar empirical approach to this chapter, Gordon and Ruffini (nd) examine non-academic outcomes and find CEP reduced suspension rates among white, male elementary students. My results are consistent with CEP providing larger benefits for younger students, as well as those living in areas with greater unmet need.

The present study builds on the existing literature in three ways. First, it provides national-level estimates of schoolwide free meals by examining changes in district performance across the entire country. To the extent that state- and district-level evaluations reflect idiosyncratic local decisions, these national results are arguably more generalizable for policymakers contemplating program reforms. Second, by exploiting variation in the timing of adoption, rather than participation decisions, this paper relies on the identifying assumption that the timing of implementation, rather then whether to implement, is uncorrelated with potential gains. Finally, it broadens our understanding of which outcomes and student groups stand to benefit from schoolwide free meal programs by exploring heterogeneous treatment effects by student and area characteristics.

2.3 Policy background: Community Eligibility Provision

CEP program details

The Community Eligibility Provision (CEP) is the largest schoolwide free meals program. In the 2019 school year, more than a quarter of school-aged children attended a CEP school

(Food Research and Action Center, 2019).[3] CEP eligibility is based on a school or district's "identified student percentage" (ISP), the share of students who receive another form of income-based assistance, primarily SNAP.[4] Schools and districts with an ISP of at least 40 percent can choose to adopt CEP, and within a district, a subgroup of schools can "pool" ISP and elect to receive CEP as a "group."[5] Over the 2012-2015 period, approximately 60 percent of participating districts fully participated, and slightly more than half of students attended a CEP school in partially-participating districts.

Important for my identification strategy, districts became eligible to implement CEP at different times over a four-year window. The rollout order was based on state and determined by the Secretary of Agriculture to ensure "an adequate number and variety of schools and [districts] that could benefit from [CEP]". Districts in Illinois, Kentucky, and Michigan became eligible to participate in the 2012 school year; districts in the District of Columbia, New York, Ohio, and West Virginia were newly eligible in 2013; districts in Georgia, Florida, Maryland, and Massachusetts became eligible in 2014; and districts in the remaining states became eligible in 2015 (Figure 2.2).

Among eligible districts, about one-third participated by 2015, ranging from 0 percent in New Hampshire to 81 percent in Montana (Neuberger et al., 2015). CEP participation has also increased within states over time. For example, in my sample, approximately 5 percent of districts in Kentucky, Illinois, and Michigan had at least one participating school in the first year of eligibility (2012). By the fourth year of eligibility in 2015, this figure had increased to 18 percent.

Both financial and student eligibility considerations affect a district's participation incentives. The federal government reimburses CEP participants at 1.6 times ISP, up to a maximum of 100 percent. For example, a district with an ISP of 40 percent receives federal reimbursement at the free meal price for 64 percent of the meals served. The remaining 36 percent are subsidized at the paid price. Since CEP schools forgo revenue from students who previously received paid meals, local sources cover any remaining costs, and these additional costs to districts reduce participation incentives. In contrast, areas with an ISP of at least 62.5 percent receive the full federal subsidy for all meals. Beyond 62.5 percent ISP, districts receive full federal reimbursement under CEP, but the financial benefit of CEP decreases

[3]Earlier schoolwide meal options include Provisions 1-3 which provide reimbursement according to base year shares of FRP students (USDA, 2002). These options are most beneficial to schools where nearly all students are income-eligible. For districts previously implementing Provisions 1-3, CEP did not change free meal access, but provided an alternative federal reimbursement. If schools aim to maximize federal revenue, Provision 1-3 schools that take-up CEP should experience a (weak) increase in federal revenue. These districts are included in my analyses only if they adopted CEP by 2017 and had a baseline FRP eligibility rate below 57.9 percent between 2009 and 2011 (e.g.: were not operating universal programs prior to 2011).

[4]Students receiving TANF or the Food Distribution Program on Indian Reservations, or who are foster youth, runaway youth, homeless, or migrants are also included in ISP.

[5]ISP is also referred to as "categorically-eligible" share or the fraction "directly certified". To see how schools may "pool" ISP, consider the following example: if one school in a district has an ISP of 20 percent and another (with equal enrollment) has an ISP of 60 percent, the two schools can combine ISP and be treated as a CEP participant with 40 percent ISP.

since these districts were already receiving a high reimbursement rate under the original program.

In addition to potentially changing federal reimbursement, CEP increases the number of students with access to free meals. Schools and districts with the lowest baseline eligibility rates experience the largest increases in access. For example, a school with a 64 percent FRP share under the traditional formula would increase free meal access by 56 percent (36 percentage points) under CEP. On the other hand, a school with a 100 percent FRP share would see no change in access (regardless of ISP).

These incentives shaped participation decisions. During the pilot period, administrators in both participating and non-participating districts cited financial concerns or reimbursement rates as one of the three most important factors in deciding whether to participate, and approximately 80 percent stated that CEP would increase access to healthy foods (Logan et al., 2014). Empirically, Figure 2.3 shows that while districts with a higher baseline eligibility rate are most likely to participate in CEP, but even among the highest-poverty schools, only about 60 percent participate. More generally, as CEP districts have systematically higher baseline eligibility rates than those that do not participate (both those eligible and ineligible), participating districts are unlikely to be a random sample. In order to compare districts with similar observable and unobservable incentives to participate, my main specifications restrict the sample to districts with any school participating in CEP by 2017 and compare districts that adopted CEP relatively early to those that adopted later.

Conceptual framework

There are several channels through which schoolwide free meals may affect average district academic performance. First, income-eligible students who do not complete enrollment paperwork gain access to the program. Second, higher-income students become eligible for free meals. For both of these groups, universal free meals increase family resources available for other food expenditures and consumption goods, which may benefit children.

Third, students' classroom experiences depend both on their own behavior and their peers' actions and classroom disruptions reduce the learning time of all students (Lazear, 2001). The literature shows that food insecurity is associated with worsened externalizing behaviors (Alaimo et al., 2001) and disruptive peers lead to worsened labor market outcomes for other students (Carrell et al., 2018). Therefore, if CEP lowers food insecurity, it may improve behaviors or reduce classroom distractions, increasing effective teaching time and benefiting students whose nutritional consumption does not change.

Fourth, since all students receive free meals in CEP schools, family income may become less salient, or consuming a school meal may become less stigmatizing, resulting in a more inclusive learning environment. Early focus groups suggest stigma reduced school meal consumption among income-eligible students (Glantz et al., 1994), and previous work examining the introduction of free meals in New York City finds increased participation among all students regardless of a student's initial FRP eligibility, consistent with universal meals reducing stigma (Leos-Urbel et al., 2013; Schwartz and Rothbart, 2017).

Fifth, CEP may lower schools' administrative costs by reducing the need to track individual free meal eligibility and participation. Districts may reallocate these cost savings to resources that directly improve student performance.[6] Although I am unable to fully disentangle among these five channels with district-level data, results are very similar to the baseline specifications when controlling for district personnel resources and revenue, indicating the findings are not solely due to changes in financial resources.

2.4 Measuring CEP participation and achievement

CEP participation

I combine information from several sources to estimate the effect of CEP on student performance. I obtain CEP participation data for public and public charter schools from state educational agencies for the 2012 through 2014 school years and the USDA Food Research Action Center (FRAC) for the 2015 through 2017 school years. Within districts, there is some variation across grade levels in CEP adoption. Elementary schools have higher participation rates than middle or high schools: In districts with any CEP adoption, about 97 percent implemented CEP in at least one elementary school and about 80 percent implemented in at least one middle school.[7] In order to obtain a district measure of participation specific to each grade, I aggregate the yearly school-level participation information to the district-grade-year level.

Student performance

In order to obtain a measure of academic performance that is comparable across states and over time, I use a novel dataset from the Stanford Education Data Archive (SEDA). These data address several issues that have precluded sub-state comparisons of student achievement. In particular, data from the biennial National Assessment of Educational Progress (NAEP) does not include all schools and the universe of NAEP-tested schools changes each survey year. Both of these features limit comparsions of performance across

[6] Although administrative costs may fall under CEP, the program's effect on net district revenue is ambiguous. There are two parameters shaping financial incentives, depending whether districts aim to maximize federal revenue or total nutritional assistance revenue (from students plus the federal government): ISP and FRP shares. First, districts with ISP rates below 62.5 percent receive less than 100 percent federal reimbursement and lose revenue from students who previously received school meals at the paid price. Second, districts with a FRP-ISP ratio above 1.6 receive less federal funds under CEP than the traditional program. Participation is expected to be lower for districts with either a FRP-ISP ratio above 1.6 or an ISP below 62.5 percent. Among such districts that do participate, the higher costs of the meals program may reduce funds available for other educational services. On the other hand, districts with an ISP of at least 62.5 percent are weakly better off under CEP.

[7] Based on data from the state of Maryland, most districts with incomplete participation would financially benefit from additional CEP coverage through 2019, suggesting strategic applications are a negligible concern over the analysis period.

districts or schools over time. Second, school-level proficiency data required of most states under the No Child Left Behind (NCLB) and the Every Student Succeeds Act (ESSA) are unreliable for cross-state comparisons, as each state designs its own test and proficiency metric, both of which substantially changed over the CEP implementation period.[8]

The SEDA data overcomes many of the limitations of the NAEP and NCLB data by using information from both sources. First, restricted-use, school-level NCLB proficiency data for the 2009 through 2015 school years are aggregated to the district-grade-year level. As detailed in Reardon et al. (2017) and Reardon et al. (2018), the SEDA approach then estimates a continuous proficiency measure for each state-subject-grade-year and by subgroup using heteroskedastic or homoskedastic ordered probit models. Each state-year-subject has a different mean and standard deviation in order to account for differences in state proficiency examinations over time and across states. These state-level distributions are then placed on the national NAEP performance scale in order to provide an achievement measure that is comparable across over time at the sub-state (district) level.[9] Finally, each subject-grade-year performance distribution is standardized to have a mean of zero and standard deviation of one. Intuitively, these data apply the within-state-year proficiency distributions from state examinations to the cross-state performance measures provided by the NAEP data. Districts in states that perform better on the NAEP examination receive a higher score in the SEDA data, as do districts that perform relatively well on their state's assessment. Reardon et al. (2017) and Reardon et al. (2018) provide a more technical treatment.

In total, the SEDA data include approximately 64,000-69,000 district-grade-year math and reading performance observations where at least one school serving grade g participated in CEP at any point through 2017.[10] My main analyses focus on a subset of about 32,000-34,000 district-grade-year observations with baseline district free meal eligibility rates lower than the median among all CEP districts (57.9 percent).

Other data

The SEDA achievement data is linked to a rich set of baseline area economic and demographic characteristics from the 2006-2010 American Community Survey (ACS). I merge these data to county unemployment rates and per-capita income maintenance payments and district school-aged poverty rates and expenditure composition in order to account for additional time-varying area and school characteristics that might affect student performance and CEP participation.

[8]Between 2012 and 2017, 44 (45) states changed their math (reading) proficiency metric at least once.

[9]Estimates for cells where the NAEP is not administered (e.g.: even numbered years and grades 3 and 5 through 7) are linearly interpolated and extrapolated.

[10]The SEDA data reports performance metrics for cells containing at least 20 assessment observations in each group. For example, black achievement measures are only available for district-grades with at least 20 black students; white-black gaps are only available for districts in which there are at least 20 white and 20 black students. Racial gaps are measured according to the standardized mean difference between the distributions for each race/ethnic group.

Empirical strategy

In order to examine how CEP affected student academic achievement, I estimate a panel weighted least squares (WLS) differences-in-differences specification, comparing districts that adopted CEP at different points between 2012 and 2017.[11] Districts in which no school chose to participate in CEP are excluded from the analysis. Among ever-participating districts, whether a district is treated in a given year depends on the state in which it is located and the first year any school serving grade g adopted CEP. Districts that first adopted in 2016 or 2017 are treated for zero years, while districts in Illinois, Michigan, and Kentucky that adopted the first year of the pilot period are treated for four years. I estimate results separately for math and reading performance with the specification:

$$y_{dgt} = \beta CEP_{dgt} + X'_{dgt}\gamma + \theta_g + \theta_d + \theta_t + \epsilon_{dsgt} \tag{2.1}$$

Where y_{dsgt} is the achievement score in district d in grade g at time t, expressed in standard deviation units. My preferred specifications focus on a dichotomous treatment where CEP_{dgt} is equal to 1 if any school serving students in grade g in district d participated in CEP in year t.[12] X_{dct} is a vector of time-varying district-grade characteristics that may be correlated with either student performance or district-level decisions to participate in CEP, including the fraction of students who are Hispanic, black, or English-learners in the district; the fraction of Hispanic and black students attending CEP schools; racial and ethnic dissimilarity indices measuring segregation patterns; the student-teacher ratio; county unemployment rates; whether the district is located in a state that is CEP-eligible in year t; and district child poverty rates. Finally, θ_g, θ_d, and θ_t are vectors of grade, district, and year fixed effects, respectively, accounting for factors that do not change within a district or grade over time, and factors that change over time, but affect all states. For example, time fixed effects account for changes in school meal nutritional requirements that applied to all states in 2013. The main analyses stack all grades in order to maximize sample size and statistical power. In sensitivity analyses, I explore whether benefits are concentrated among younger or older students.

Since the sample is limited to districts that participated in CEP by 2017, a causal interpretation of these results requires that the timing of CEP participation is uncorrelated with potential performance, conditional on fixed district factors and time-varying observable characteristics. This assumption would be violated if pilot states were chosen based on potential benefits of CEP adoption, or if districts chose to implement CEP at a point that was most advantageous to student performance. Both policy details and baseline characteristics

[11] Following the recommendations in the SEDA documentation, all performance outcomes are weighted by the inverse of the squared standard error of the mean. Columns (5) and (6) of Appendix Table B.5 shows the main math results are robust to unweighted and student-enrollment-weighted models.

[12] Column (5) of Appendix Tables B.5 and B.6 shows smaller and less precise results when defining treatment as the share of students in a CEP school. In Section 2.5, I show this pattern is due to high rates of partial participation among districts with low baseline FRP eligibility rates.

can inform the plausibility of this identifying assumption. In addition, Section 2.5 formally explores this hypothesis with an event study approach.

From a policy perspective, legislation limited the number of pilot states to three in 2012 and four in 2013 and 2014. In determining which states were selected, the Secretary of Agriculture was instructed to "select states with an adequate number and variety of schools and [districts] that could benefit from [CEP]" (Public Law 111-296). In determining the 2012 pilot states, USDA identified states with the greatest number of schools that were likely to qualify based on SNAP participation rates, and allowed ten states to apply (USDA 2011).[13] The selection criteria changed the following two pilot years: all states could apply and states were chosen based on knowledge and awareness of CEP procedures and likely take-up (USDA, 2012, 2013). Baseline academic performance was not a formal criterion in selecting the pilot states, and of the seven states that were eligible to apply but were not selected in 2012, only DC was subsequently chosen as a pilot state.

Examining district baseline characteristics can also suggest whether the timing of CEP participation is correlated with factors that may affect changes in student performance. Figures 2.4, 2.5, and 2.6 display baseline (2009-2011) area economic and district characteristics by year of CEP adoption. In each figure, the solid line shows the distribution of districts that adopted CEP prior to 2016; the dotted line shows the distribution of districts that adopted in 2016 or 2017; and the dashed line shows the distribution of districts that were not participating in CEP as of 2017. These figures show districts with at least one CEP-participating school are more disadvantaged than districts with no participation: prime-age labor force participation rates and median income are lower, and child poverty, income inequality, baseline FRP eligibility, and unemployment rates are higher. Looking at student characteristics, CEP districts tend to have larger shares of black and Hispanic students, and worse academic performance. Differences between early- and late-adopting districts, however, are more muted, suggesting early-adopting districts are more similar to late-adopting areas than never-adopting districts.

The differences in Figures 2.4, 2.5, and 2.6 suggest CEP districts are not randomly selected and motivate restricting attention to ever-participating districts. Table 2.2 explores whether area and district characteristics vary among CEP districts across year of implementation.[14] While there are some notable differences – in particular, the initial CEP cohorts have fewer Hispanic students, and the 2012 cohort is more disadvantaged, economic conditions are not differently trending for the earliest adopters and results are robust to excluding districts that adopted the first pilot year (results available upon request).

While these details suggest much of the timing of CEP eligibility was orthogonal to student performance trajectories, it is possible that states with the greatest awareness of the program and relatively well-organized state efforts were selected earlier. If state organization or activity is correlated with both pilot status and achievement trends, leveraging only vari-

[13]These states were Alaska, DC, Illinois, Kentucky, Louisiana, Michigan, Mississippi, Oklahoma, South Carolina, and Tennessee.

[14]The 2016 and 2017 adoption years are combined for brevity, as districts adopting in each of these years are untreated throughout the analysis period.

ation in state-level eligibility timing would lead to biased results. On the other hand, if pilot selection was unrelated to factors shaping student performance, but districts participated in CEP in response to potential student benefits, a participation-based treatment definition will be biased. In practice, disentangling pre-eligibility trends from secular trends is challenging in this setting, as there are only four eligibility "waves" with the vast majority of districts becoming eligible in 2015. With this caveat in mind, additional analyses suggest that states became eligible for CEP during a period coincident with worsening performance. On the other hand, leveraging both within- and across-state variation allows me to incorporate a rich set of state-specific trends and controls in order to account for state-level factors shaping participation decisions.

To evaluate and account for factors that may affect both the timing of participation and student achievement, I conduct three complementary analyses. First, the main empirical approach controls for all time-invariant district characteristics, as well as many time-varying observable factors that are correlated with CEP participation and performance – such as child poverty rates, the unemployment rate, and the racial/ethnic composition of schools and districts. Second, Section 2.5 presents event study analyses indicating that there are no significant pre-trends in academic performance for black and Hispanic students prior to CEP adoption after conditioning on district characteristics. Third, I test robustness to a series of standard modifications and extensions, such as exploiting only within-state variation or including linear time trends in baseline variables, following the approach of Hoynes and Schanzenbach (2009) and Hoynes et al. (2016). Findings for math performance are robust to each of these extensions.

2.5 Results

Only districts with high FRP eligibility are able to participate in CEP. In particular, participating districts must have at least one school with an ISP (and thus, baseline FRP eligibility rate) of at least 40 percent. In practice, many participating districts have baseline eligibility rates well above the minimum threshold: on average, about 58 percent of students were eligible for free meals before CEP, and in 10 percent of CEP districts, more than 80 percent of students were eligible (Figure 2.7). Recall that the switch to CEP did not substantially change free meal access in districts with relatively high baseline eligibility rates as most students were already eligible for free meals. On the other hand, districts on the eligibility cusp – those with a FRP rate just above the 40 percent ISP threshold – saw free meal access increase up to 60 percentage points under CEP. Therefore, any treatment effect – in terms of both free meal consumption and performance – is likely largest in districts and schools with relatively low baseline eligibility. To examine heterogeneity by the effective treatment "dose", I partition the sample of CEP-adopting districts at the median baseline eligibility rate (57.9 percent). Districts with a baseline eligibility rate less than 57.9 percent form the "exposed" subsample for which CEP led to the largest increases in free meal access.

CEP and School Meal Participation

In order to establish that CEP affected meal consumption, I collect administrative school-level meal count data in six of the eleven states that adopted CEP before 2015: Georgia, Illinois, Kentucky, New York, Maryland, and West Virginia. Data availability varies by state, and in total, the meal participation data cover approximately 18,800-20,000 school-year observations spanning 2009 through 2016. I merge the meal count data to enrollment information from the Department of Education's Common Core of Data and school-level CEP participation in order to obtain a per-student measure of consumption before and after CEP adoption.

This chapter is the first to provide a direct measure of meal consumption for multiple states. One important limitation, however, is that meal count data are not available for all states. I therefore supplement the consumption analyses with information on federal funding districts receive for the school meals program from the Department of Education's School Finance Survey. While the finance data is available for every district in the country, one noteworthy shortcoming is that the reported revenue amount conflates changes in the quantity of meals with changes in the per-meal subsidy rate, both of which are expected to change under CEP.

Table 2.3 presents the main consumption results from estimating the panel differences-in-differences specification in Equation 2.1 (at the school level for meal consumption, and district level for federal nutritional assistance funding). Column (1) indicates CEP increased the number of breakfasts served among all CEP schools by 20 meals a student a year (about 38 percent). The change among schools in the exposed district subsample is comparable in both number of meals served and the proportional increase (column (2)). Columns (3) and (4) show the number of lunches increased by 12-13 per student a year for both samples (about 12 percent). Consistent with CEP increasing meal consumption, per-student federal school meal revenue increased by approximately 9 percent (columns 5 and 6). While I find the changes in per-student meal consumption and federal reimbursement are similar in the exposed subsample to the full sample of schools, not all states maintain breakdowns by subsidy rate, and I am unable to fully decipher whether these patterns are due to increases in the number of free meals offsetting reductions in the number of paid meals, or what types of students increase their meal consumption.

In the case where schools and districts adopt CEP in response to increased student demand for school meals, the differences-in-differences regression results in Table 2.3 would overstate the effect of CEP on meal consumption. In order to investigate whether these findings are the continuation of longer-term trends in school meal participation, as well as how participation evolves after implementation, Figure 2.8 displays an event study analysis taking the form:

$$y_{spt} = \sum_{p=-5}^{2} [\beta_p \mathbb{1}(P_{spt} = p)] + X'_{spt}\gamma + \theta_s + \theta_t + \varepsilon_{spt} \tag{2.2}$$

for annual per-student meal consumption y_{spt} in school s p years after the first year of CEP adoption in calendar year t. $\mathbb{1}(P_{spt} = p)$ are a series of indicator variables p years after the first year of implementation; β_p traces out changes in school meal consumption for the full event window, with the year before CEP implementation, β_{-1}, normalized to zero.

Panel (a) shows that for the exposed subsample the number of breakfasts per student was not significantly trending before CEP implementation, and discretely jumped by about 10-20 meals a student a year once CEP was offered.[15] Results for school lunches show that schools tended to implement CEP after lunch participation had been increasing for several years, suggesting that schools may have responded to increasing demand by expanding access to the entire student body. Importantly, however, parametric event studies show a large and strongly significant trend break coinciding with the year of CEP adoption for both breakfasts and lunches, and all samples and specifications (Appendix Table B.1). The estimated increase in lunch participation is about 10 meals per student per year (columns 5-8), only slightly smaller than the differences-in-differences results in Table 2.3.[16]

CEP and Academic Performance

Achievement results

Although greater access to free school meals increased school breakfast and lunch receipt, this consumption may not translate into changes in academic performance as the existing literature finds mixed results of the traditional meals program on academic performance. Schoolwide free meal programs tend to yield more systematic benefits, but these results are somewhat sensitive to how meals are provided and the population studied.

Starting with the full sample of all CEP-participating districts, Table 2.4 shows CEP did not improve overall reading or math performance. Column (1) estimates Equation 2.1, including district, cohort, and year fixed effects, but without controlling for time-varying district or economic conditions. Columns (2)-(5) add these characteristics and examine performance among racial/ethnic subgroups. Across specifications and subgroups, there is no significant improvement in math or reading performance.

As the effective treatment "dosage" under CEP depends on a district's baseline eligibility and participation in the free meals program, with the highest-poverty districts experienced little effective change under CEP, the remaining analyses focus on the "exposed" subsample of districts with baseline eligibility rates below the CEP sample median of 57.9 percent. CEP increased free meal access an average of 32 percentage points in these districts, substantially

[15] For the full sample, the pre-period coefficients are jointly significant at the 10 percent level, but both parametric and non-parametric event studies show a discrete increase of at least 10 meals following CEP implementation.

[16] Appendix Table B.2 augments the differences-in-differences results with state-specific trends. Under this approach, increases in breakfast consumption are somewhat attenuated (12-13 meals per student), and lunch consumption is similar to the main results in Table 2.3.

higher than the 23 percentage point increase for lower-exposure (higher baseline eligibility) districts.[17]

The subset of exposed districts differs in several important respects from CEP districts with higher baseline FRP eligibility, summarized in Table 2.5. First, by definition, these districts had fewer students eligible under the traditional program than non-exposed CEP districts. Similarly, economic conditions – measured by median income, child poverty, and unemployment rates – are slightly better in the exposed subsample. Second, exposed districts have slightly smaller minority shares, and had higher math and reading performance prior to CEP. Third, in exposed areas, participation decisions are less likely to be made at the district level. Whereas 70 percent of non-exposed participating districts had full district participation, 57 percent of exposed districts fully participated by 2017. All of these patterns suggest the exposed sample consists of relatively low-poverty districts (compared to other CEP districts. In an absolute sense, even low-poverty CEP districts tend to be more disadvantaged than non-participating districts).

Focusing on the subsample of exposed districts with the largest gains in access to free meals indicates important treatment heterogeneity: for this group, CEP improved overall math performance by about 0.02 standard deviations (Table 2.5, column (1)). Scaling the this intent-to-treat estimate by the fraction of students gaining access to free meals (32 percent) implies that free school meals improve overall math performance by approximately 0.05 standard deviations.

These improvements in math performance are concentrated among populations that are especially likely to gain access to free meals under CEP. In particular, income-eligible Hispanics have lower participation rates than other groups (Chaparro et al., 2014; Goerge et al., 2009), while white students tend be less likely to qualify on the basis of income. The remaining columns of Table 2.5 show Hispanic math performance by approximately 0.03 standard deviations (column (3)), and white math performance about 0.02 standard deviations (column (4)). In contrast, math performance among black students did not significantly change. Therefore, column (5) suggests while white-black performance gaps widened following CEP, this measure is driven by improvements in the absolute performance of white students, rather than deteriorating performance among black students.

Panel (b) presents analogous results for reading. Across subgroups, reading outcomes are smaller in magnitude and less precisely estimated. A series of robustness checks also demonstrates the magnitude and sign of reading performance is sensitive to the specification and sample. Given these patterns, the remaining analyses focus on math performance; reading results are included in the Appendix for completeness.

The reduced-form estimates in Table 2.5 provide the intent-to-treat (ITT) effect of offering free meals to all students. Recovering the effect of actual meal consumption on treated students (TOT) is not feasible with district (or school) aggregate performance measures.

[17]While this increase in access is larger than the change in the number of lunches served from the meal participation data, recall that the consumption data includes free, reduced, and paid meals. Under CEP, some students who previously purchased a school meal would continue to receive a school meal, but would have no out-of-pocket costs.

In principle, instrumenting the change in meal consumption by CEP participation would obtain the district-level TOT. In practice, however, meal data are only available for schools in six states, and only half of these observations contain breakdowns by payment category. To overcome these data limitations, I consider how the change in federal funding for school meals induced by CEP participation affects student performance.

Appendix Table B.3 reports results from instrumenting per-student federal school meal spending with CEP participation, and shows that an additional $1,000 per student in school meals induced by CEP adoption increases math scores by an insignificant 0.16 standard deviations for the full sample (column 1) and 0.51 standard deviations for the exposed subsample. Reading does not significantly improve in either sample. As the average district received approximately $100 in additional per-pupil school meal reimbursement under CEP, this implies access to schoolwide free meals improved math scores by 0.05 standard deviations, consistent with the scaled results in Table 2.5. Interestingly, while average changes in federal funding are similar for both samples (about $100), improvements are concentrated in the exposed subsample. This pattern suggests that not only revenue amounts, but also which types of students benefit from additional resources, is important for understanding changes in average performance.

Timing of CEP adoption

The differences-in-differences specifications provide an estimate of the average effect of CEP one to four years after implementation, relative to previous years. One outstanding question is how districts were performing prior to CEP, and whether any benefits grew or diminished with program experience. To this point, event study analyses can illustrate the extent to which student performance changed over the full analysis period, and whether CEP adoption coincides with previous trends in student performance. Figure 2.9 displays event study plots for math performance by estimating Equation 2.2 at the district level. These plots show math performance was not significantly trending for black (panel b) or Hispanic (panel c) students prior to CEP adoption. However, CEP adoption followed a period of improvements in white students' math performance (panel d). Reflecting the fact that white students account for a large share of the total student population, there is suggestive evidence of improvements in overall performance over the longer term (panel a). Appendix Figure B.1 shows these general patterns are similar after including state linear trends and trends in baseline variables. In contrast, reading performance does not display any significant pretreatment (or post-treatment) pattern for any subgroup (Appendix Figure B.3). If anything, black performance was slightly worsening prior to CEP adoption.[18] Given these patterns, I emphasize results for white students should be interpreted cautiously, as districts tended to adopt CEP during a period of secular improvements in performance. In contrast, the timing of CEP implementation is more plausibly exogenous from the perspective of black

[18] Patterns are similar with the inclusion of state and baseline variable trends.

and Hispanic math performance.[19]

The analyses in Figures 2.9 and Appendix Figure B.3 display unbalanced event studies, binning all years earlier than -5 (years -8 through -5 for the 2017 cohort) and years later than 2 (years 2 and 3 for the 2012 cohort) in order to provide suggestive evidence the extent to which benefits from schoolwide free meals cumulate or diminish over time. Although point estimates generally suggest greater achievement gains with each year of access, I cannot reject equal treatment effects in each of the first three years of participation.[20] As more districts gain experience with schoolwide meals, greater exploration of this topic can inform the extent to which there are diminishing marginal returns to each year of access.

Extensions: Heterogeneity and Sensitivity analyses

Performance by grade level Previous work has found that universal meal programs have particularly large benefits for young children (Gordanier et al., 2019; Fuller and Comperatore, 2018; Gordon and Ruffini, nd). Consistent with the existing literature, Table 2.6 suggests that CEP improved math performance significantly more for black and Hispanic elementary (grades 3-5) than middle (grades 6-8) schoolers.

District resources and student composition By changing the federal school meal reimbursement formula, CEP adoption may have affected performance by altering district revenue or resources, and any changes in district resources may contribute to changes in academic performance. Table 2.7 explores this possibility by examining various resource measures and shows that total federal revenues (column (1)), federal revenues net of nutritional assistance payments (column (2)), and total per-pupil expenditures (column (3)) did not significantly change following CEP adoption. Columns (4) and (5) show that per-pupil instructional expenditures and student-teacher ratios slightly fell, suggesting districts increased the number of instructional staff at lower salaries.[21] These changes in resources, however, do not drive the main findings. When controlling for per-student total and instructional expenditures, results are nearly identical to the baseline results in Table 2.5 (Appendix Table B.5, column (1)).

Since free school meals provide an in-kind subsidy to families (with a fungible value of about \$4.50 a school day), CEP adoption may have changed the student composition of a district if families moved into adopting districts or transferred from private schools to public

[19] Limiting the source of identifying variation to the year of eligibility is likely problematic in this setting as both math and reading performance were steadily and significantly worsening for most subgroups before their state became eligible to adopt CEP. These patterns call into question the exogeneous nature of eligibility decisions, at least with respect to student performance, but due to the short nature of the phase-in period, it is difficult to disentangle these patterns from secular trends in performance.

[20] While only the earliest and latest-adopting districts contribute to the tails in these figures (and these districts account for a small share of all CEP districts), balanced event study plots show qualitatively similar patterns (Appendix Figure B.4).

[21] In additional results, I find no statistically or economically significant change in district-grade enrollment.

schools. To explore whether CEP changed district composition, Table 2.8 panel (a) regresses district-level student characteristics on CEP participation. These results show no changes in the share of black or white students enrolled in a district following CEP implementation, and a slight reduction in the share of Hispanic students (0.3 percentage points, or about 2 percent).

Even with minor shifts at the district level, CEP could prompt intra-district sorting or changes in segregation patterns if districts realigned school boundaries in order to maximize CEP eligibility or if students transferred in or out of CEP schools based on perceived benefits. Such sorting is mostly likely to occur in districts where some, but not all, schools adopt CEP. Panel (b) of Table 2.8 examines school-level demographic shifts for the subset of districts in which at least one school in the district participated in CEP and at least one did not by measuring the fraction of students in each racial/ethnic group attending CEP schools. In these districts, there is no economically or statistically significant change in the fraction of students from any racial/ethnic group attending a CEP school. Looking more directly at segregation patterns, panel (c) suggests CEP slightly increased the concentration of white and black students in a school, measured by district-grade dissimilarity indices.

All of the previous analyses controlled for segregation patterns and student demographics at both the district and school level in order to account for shifts in student composition. As a complementary exercise to place bounds on the extent that changes in the student population can account for observed changes in performance, Appendix Table B.4 defines predicted performance for each subgroup-subject as the grade-specific fitted values from regressing district segregation and student composition on performance. While point estimates suggests demographic shifts coinciding with CEP would lead to improved performance for Hispanic and white students, these demographic changes can explain no more than ten percent of the observed improvements in math performance.

Alternative specifications and sample definitions Appendix Table B.5 explores the robustness of improvements in math performance. As mentioned previously, results are unchanged when including controls for financial resources, suggesting that changes in district resources are not driving the main results (column (1)). Columns (2) and (3) add state-specific linear trends and trends in baseline child poverty, unemployment, student racial/ethnic composition and segregation, and student-teacher ratios in order to account for possible performance trends that are correlated with the timing of implementation, following the approach in Hoynes et al. (2016). Column (3) additionally controlls for prior-year performance. While effects for black and white students are sharply attenuated under this approach, estimates for Hispanic students are relatively unchanged. Column (4) includes state-by-year fixed effects, thereby only exploiting variation in CEP adoption within a state. This specification effectively pools many (51) single-state analyses, and again, results point to improvements in math performance, particularly among Hispanic students.

During the 2012 through 2017 period, about 60 percent of participating districts had full participation, and more than 75 percent of students attended a CEP school in an additional 13 percent of districts. Given this distribution, the main results define treatment as a binary indicator, regardless of the share of students attending CEP schools. An alternative approach would define a district's "treatment" as the share of students attending CEP schools. Under this approach in Appendix Table B.5 column (5), results are smaller in magnitude and much less precise, but confidence intervals cannot rule out improvements of the magnitude shown in Table 2.5. The differences between estimates using a binary and continuous treatment variable suggest districts with partial participation experienced the largest benefits. I return to this issue in Section 2.5.

Finally, columns (6) and (7) return to the main estimating equation (Equation 2.1), but instead of implementing weighted least squares, these columns weigh the results either equally across districts (column (6)) or by the log of baseline student enrollment (column (7)). In both cases, estimates are similar to the main findings, suggesting that any benefits are not concentrated in particularly large or small districts. Appendix Table B.6 presents the corresponding results for reading performance. Here, findings are less consistent across specifications. When even a parsimonious set of additional controls are included, I cannot rule out no effect of CEP on reading performance for any subgroup.

I also explore robustness to different sample definitions in Appendix Table B.7 (math) and B.8 (reading). As each of these alternative samples is small relative to the main results, the loss of statistical power precludes making definitive conclusions and these results should be viewed as suggestive. Column (1) limits the sample to districts with full participation – districts in which the binary treatment measure coincides with the fraction of students attending a universal free meal school. Moreover, student sorting across schools in response to CEP is less likely to occur in these districts. Although point estimates are attenuated, confidence intervals cannot rule out changes in performance comparable to those reported in Table 2.5. Column (2) focuses on the balanced panel of districts that have a valid observation for each year covered in the SEDA data to assuage concerns that the main findings – particularly for race/ethnic groups – are driven by changes in the number of students of a race/ethnic group enrolled in the district, or by the inclusion of states that experienced changes in their state examinations over this period.[22] Again, results point in the same direction as the main results. Column (3) lastly limits the sample to districts that adopted CEP the first year their state became eligible. In these districts, CEP timing is driven by statutory eligibility, rather than district-level decisions. While these estimates cannot reject the null hypothesis that CEP had no effect, they also cannot rule out changes of the magnitude reported in Table 2.5. Appendix Table B.8 again illustrates the sensitivity of the reading results, and in general, there is no systematic evidence CEP improved reading among any student or district subgroup.

Finally, Appendix Figures B.5 and B.6 plot coefficients and confidence intervals from the specifications in Table 2.5, but dropping a single Census Division, state, or grade in order to explore whether treatment effects vary by geography or grade level. These figures illustrate that results are not driven by the experiences of a single geographic area, and consistent

[22] Reardon et al. (2018) details the methodology for censoring and excluded observations. The most common reason for exclusion is a substantial change in a state's examination.

with Table 2.6, younger students tend to experience larger math improvements.

Effects throughout the exposure distribution

The main analyses focus on the set of CEP districts below the baseline free meal eligibility median (57.9 percent). Across the full sample of CEP-participating districts, however, there is wide variation in baseline FRP eligibility: About 12 percent of CEP districts have a baseline eligibility below 40 percent, and 10 percent have baseline eligibility above 80 percent (Figure 2.7).[23]

Recall that "exposure" is defined as $1 - pctFRP_{dg,2009-2011}$: the share of students in district d, grade g *ineligible* for free and reduced meals between 2009 and 2011. If all schools in a district participate in CEP, exposure is equivalent to the share of students gaining access under CEP. This is the case for about the 60 percent of CEP districts with full participation. In districts with partial participation, the share of students gaining access under CEP is less than a district's exposure. In addition, the likelihood that all schools within a district participate is increasing in district baseline FRP eligibility, illustrated in Figure 2.10, panel (a). Accordingly, while the share of students with access to free meals (either through the traditional formula or CEP) is increasing in baseline eligibility (panel (b)), there is an inverse-U shape relationship between baseline eligibility and the fraction of students *gaining* access under CEP – the effective "treatment" dosage. Figure 2.10 panel (c) shows that access gains are largest for districts with baseline poverty shares between about 50 and 60 percent. The highest-exposure schools (those with the lowest baseline FRP eligibility) experience relatively little effective treatment under CEP as relatively few schools in the district participate; and the lowest-exposure schools also experience essentially no gains due to high baseline eligibility.

Mirroring the distribution of increased eligibility, there is a non-monotonic relationship between exposure to CEP and performance gains, both in the the aggregate and for Hispanic students, shown in Table 2.9. Although not statistically different across the distribution, point estimates suggest CEP conferred the largest benefits to Hispanic students in districts with less than 50-60 percent of students eligible for free meals at baseline. In contrast, any improvements in black math performance are concentrated in districts with the lowest baseline eligibility rates.

Appendix Table B.9 presents analogous results for reading. While point estimates suggest that any improvements are limited to the highest-income districts (those with baseline eligibility rates below 40 percent), none of these findings is statistically different from zero.

One possible explanation for these patterns is that the *students* gaining access in relatively high-exposure districts had the largest marginal benefits from additional nutritional assistance. While this hypothesis is untestable with district-level data, evidence from state studies suggests there may be heterogeneity across areas in the types of students benefiting

[23]The cut variable is defined on district baseline eligibility and does not factor in the share of schools within the district actually adopting CEP in order to allay concerns that groups of schools within districts may strategically apply for CEP in order to maximize total revenue or participation.

the most from universal provision, although the existing work has not reached a consensus on this point. In the case of South Carolina, low-income students who did not receive TANF or SNAP experience the largest improvements (Gordanier et al., 2019), while New York City students who were previously ineligible for free meals benefited the most from universal provision (Schwartz and Rothbart, 2017).

Heterogeneous effects

Besides the share of students previously eligible for free meals, the benefits of schoolwide free meals may vary with other area characteristics. Unlike programs like SNAP or TANF that provide a near-cash benefit, school meals are a quantity-based form of assistance. While the monetary value of in-kind benefits is higher in expensive areas, the additional resources from free meals increase family purchasing power by a greater amount in low-cost of living areas. With decreasing marginal benefits of consumption, additional nutritional assistance is also expected to be higher in areas where few families receive other income assistance programs (conditional on income). Finally, with the cavear that there are few urban areas in each state, several single-state papers have documented CEP tends to yield greater benefits in non-urban areas (Gordanier et al., 2019; Fuller and Comperatore, 2018).

To explore whether the benefits of schoolwide meals are concentrated in any of these areas, I partition the main analysis sample based on urban location and at the CEP-sample baseline median cost-of-living, SNAP receipt, and per-capita income assistance levels. Table 2.10 suggests that math performance improvements are concentrated in areas with relatively low costs of living, consistent with CEP providing greater purchasing power for families in these areas. More granular student-level data that includes measures of family income and consumption could provide more concrete evidence on this hypothesis. In contrast, there are no systematic differences in treatment effects by urban location or participation in other income assistance programs (columns (2) through (4)).

2.6 Discussion and conclusion

This chapter finds that schoolwide free meal programs increase breakfast and lunch participation. In addition, schoolwide free meals led to modest improvements in math performance among groups likely to gain access to free meals under universal provision, as well as for younger students. Results are not driven by concurrent changes in school resources or observable features of the school environment.

These findings are largely consistent with results from papers examining the effect of CEP in a single state, as well as findings from earlier universal breakfast programs. In South Carolina, Gordanier et al. (2019) find that CEP improved math performance among elementary by about 0.06 standard deviations, with smaller effects in reading and among middle school students. In North Carolina, Fuller and Comperatore (2018) find elementary math performance improved approximately 0.02 to 0.03 standard deviations, middle school math performance did not significantly improve, and both reading performance improved approximately 0.04 standard deviations for both grade levels. Schwartz and Rothbart (2017) find improvements of a similar magnitude (0.03 standard deviations) among students who qualified for free meals under the traditional program, and larger improvements among higher-income, previously-ineligible students. While schoolwide free meals did not significantly improve academic performance in other settings (Kho, 2018; Leos-Urbel et al., 2013), my analyses suggest that negligible aggregate effects of such programs may mask differential effects across student populations or schools, depending on the magnitude of the effective "treatment."

Importantly, prior single-state evaluations of CEP compare the experiences of schools that choose to implement CEP with those that do not. In contrast, all districts in my sample opted to participate in CEP, but differ in the timing of adoption. That these studies leverage different sources of variation, yet reach broadly similar conclusions supports an earlier body of work pointing to the role of nutritional assistance and school investments in improving short-term outcomes for students.

When interpreting these results, it is important to recall that CEP expands free meals to two types of students. One group is students who live in a high-poverty district, but whose family incomes are greater than the cutoff for free meals. The second is students who are income-eligible for the traditional program, but who are not receiving other forms of assistance and whose families did not complete the required paperwork. The literature has not reached a consensus of which *students* benefit the most from universal access. Given the aggregate nature of district-level data, I am unable to fully explore the extent to which *individual* benefits differ by receipt of other forms of assistance or family income. Scores by race and ethnicity can provide insights into this heterogeneity only if race/ethnicity is correlated with students' socioeconomic status.

While the modest improvements in math performance documented in this chapter are similar in magnitude to other papers examining the effect of CEP and related schoolwide meals programs in a single state, they are small relative to earlier district-led initiatives that modified how school meals were served. In particular, existing work suggests moving breakfasts from before school to during the school day leads to slightly larger improvements than schoolwide free meals (Dotter, 2013; Imberman and Kugler, 2014). However, considering the size and generosity of the implied income transfer, CEP offers benefits similar in magnitude to other forms of income assistance. For example, an additional $1,000 in EITC payments increases test scores for 3-8th graders by about 0.04 standard deviations (Dahl and Lochner, 2017). By these metrics, CEP delivered benefits on the order of a $500 family income transfer, for a cost to the federal government of approximately $100 a student. Taken as a whole, these results suggest that school-based assistance can yield important benefits, particularly for groups unlikely to have access to traditional, family-income based programs.

2.7 Tables and Figures

Table 2.1: Federal Reimbursement Rates for School Meals, 2017-2018

	Free	Reduced	Paid	Nutrition Quality
		Breakfast		
48 Contiguous States	\$1.66 (\$1.99)	\$1.36 (\$1.69)	\$0.29	
Alaska	\$2.66 (\$3.19)	\$2.36 (\$2.89)	\$0.43	
Hawaii	\$1.94 (\$2.32)	\$1.64 (\$2.02)	\$0.33	
		Lunch		
48 Contiguous States	\$3.23 (\$3.29)	\$2.83 (\$2.89)	\$0.31 (\$0.33)	\$0.06
Alaska	\$5.24 (\$5.26)	\$4.84 (\$4.86)	\$0.50 (\$0.52)	\$0.06
Hawaii	\$3.78 (\$3.80)	\$3.38 (\$3.40)	\$0.36 (\$0.38)	\$0.06

Notes: Source: USDA (2017). Left numbers show the base federal reimbursement rate; right numbers show the rate for high-poverty schools (for lunch, schools with at least 60 percent of students receiving free or reduced meals; for breakfast, schools with at least 40 percent of students receiving free or reduced meals). In addition, schools receive an additional 6 cents per lunch for serving fruits and vegetables.

Table 2.2: Baseline District Summary Statistics by Year of CEP Implementation

	(1)	(2)	(3)	(4)	(4)
	2012	2013	2014	2015	2016-7
Panel A: Baseline area characteristics					
% FRP	0.674	0.599	0.591	0.599	0.558
	(0.138)	(0.143)	(0.131)	(0.181)	(0.172)
Urban	0.158	0.191	0.142	0.170	0.131
	(0.365)	(0.393)	(0.350)	(0.376)	(0.337)
% college educated*	0.145	0.155	0.160	0.172	0.168
	(0.077)	(0.077)	(0.087)	(0.086)	(0.088)
Unemployment rate	0.077	0.064	0.057	0.060	0.055
	(0.034)	(0.029)	(0.019)	(0.027)	(0.026)
% single mother*	0.386	0.329	0.346	0.341	0.306
	(0.143)	(0.135)	(0.128)	(0.129)	(0.113)
Median household income ($1000s)*	40.490	44.410	45.340	46.200	49.710
	(12.890)	(10.990)	(11.400)	(13.730)	(14.970)
Gini coefficient*	0.432	0.416	0.414	0.411	0.397
	(0.060)	(0.043)	(0.051)	(0.048)	(0.052)
Child poverty rate*	0.319	0.272	0.259	0.264	0.237
	(0.127)	(0.092)	(0.090)	(0.110)	(0.104)
Per-capita income assistance ($1000s)	1.320	1.194	1.144	1.166	1.091
	(0.512)	(0.401)	(0.334)	(0.434)	(0.367)

Table 2.2: (continued)

	(1)	(2)	(3)	(4)	(4)
	2012	2013	2014	2015	2016-7
Panel B: Baseline district characteristics					
% charter schools	0.016	0.048	0.045	0.034	0.026
	(0.048)	(0.108)	(0.091)	(0.077)	(0.076)
% black	0.306	0.205	0.299	0.241	0.186
	(0.343)	(0.277)	(0.277)	(0.306)	(0.253)
% Hispanic	0.075	0.063	0.086	0.201	0.171
	(0.133)	(0.122)	(0.132)	(0.287)	(0.260)
% special education	0.172	0.171	0.148	0.138	0.141
	(0.035)	(0.037)	(0.041)	(0.057)	(0.058)
# schools	16.970	21.010	20.500	17.480	14.810
	(41.870)	(64.490)	(39.510)	(33.790)	(47.550)
Student-teacher ratio	16.060	15.680	15.750	15.480	18.640
	(2.652)	(2.321)	(3.394)	(8.145)	(154.000)
Per-pupil expend ($1000s)	13.150	14.090	12.500	13.470	13.410
	(2.324)	(3.900)	(2.789)	(4.117)	(4.296)
School meal revenue ($1000s)	0.440	0.386	0.405	0.413	0.383
	(0.109)	(0.115)	(0.117)	(0.163)	(0.140)
% full CEP participation (2017)	0.710	0.601	0.649	0.605	0.611
	(0.417)	(0.465)	(0.447)	(0.461)	(0.459)

Table 2.2: (continued)

	(1)	(2)	(3)	(4)	(4)
	2012	2013	2014	2015	2016-7
Panel C: Baseline district performance					
Overall math	-0.382	-0.326	-0.253	-0.288	-0.242
	(0.346)	(0.326)	(0.303)	(0.368)	(0.358)
Hispanic math	-0.493	-0.585	-0.306	-0.366	-0.366
	(0.331)	(0.281)	(0.279)	(0.298)	(0.301)
White math	-0.207	-0.187	-0.059	-0.004	-0.017
	(0.326)	(0.310)	(0.326)	(0.332)	(0.335)
Black math	-0.721	-0.673	-0.582	-0.586	-0.556
	(0.282)	(0.282)	(0.276)	(0.297)	(0.300)
Overall reading	-0.278	-0.242	-0.188	-0.299	-0.238
	(0.320)	(0.300)	(0.280)	(0.342)	(0.354)
Hispanic reading	-0.427	-0.495	-0.332	-0.474	-0.456
	(0.338)	(0.270)	(0.285)	(0.265)	(0.303)
White reading	-0.108	-0.112	0.012	0.022	0.010
	(0.280)	(0.307)	(0.308)	(0.292)	(0.302)
Black reading	-0.599	-0.536	-0.508	-0.543	-0.510
	(0.274)	(0.261)	(0.246)	(0.261)	(0.275)
Observations	2162	4316	4310	26630	23314

Notes: Table shows baseline characteristics by year of CEP implementation for district-grades with any school participating in CEP between 2012 and 2017. Baseline defined as 2006-2010 for data available through the American Community Survey (denoted by "$*$"), 2009-2011 for other sources. Column headers denote the first year a district-grade had any CEP participation. Unemployment rate from BLS LAUS, child poverty rates from Census Bureau SAIPE, per-capita income assistance from BEA REIS. Other area characteristics from the American Community Survey, and all district resources and performance measures from SEDA. All dollars in 2017 constant thousand dollars, adjusted for inflation with the CPI-U-RS. See text for details.

Table 2.3: CEP and Change in Breakfasts and Lunches Served

	(1)	(2)	(3)	(4)	(5)	(6)
	Per-student breakfast		Per-student lunch		Log per student nutrition asst	
	All	Exposed	All	Exposed	All	Exposed
CEP	19.873***	19.794***	13.194***	12.140***	0.091***	0.093***
	(2.582)	(3.228)	(1.188)	(1.085)	(0.009)	(0.012)
Observations	18762	12077	20030	13193	128656	64212
Baseline DV mean	52.57	49.16	111.9	104.3	0.400	0.327
% change	0.378	0.403	0.118	0.116		
Level	School	School	School	School	District	District

Notes: Table presents regression results from unweighted school-level meal count data (columns 1-4) collected from state Department of Educations for six of the eleven states that adopted CEP before 2015: Georgia, Illinois, Kentucky, New York, Maryland, and West Virginia. Data availability varies by state, but spans 2009-2016. Columns 5 and 6 presents federal nutritional assistance to districts from the Annual Survey of School System Finances ($1,000s of 2017 dollars). All specifications include controls for student demographics, the fraction of charter schools in a district, child poverty and unemployment rates, and measures of racial/ethnic segregation, as well as year fixed effects. Columns 1-4 also include school fixed effects; columns 5 and 6 include district fixed effects. Odd numbered columns ("All") include all observations that adopted CEP between 2012 and 2017; even-numbered columns ("Exposed") restrict the sample to observations in districts with a baseline FRP eligibility rate below 57.9 percent (the median among CEP-adopting districts). Robust standard errors clustered by district. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table 2.4: Effect of CEP on Academic Performance: Full Sample

	(1)	(2)	(3)	(4)	(5)
	Overall	Overall	Black	Hispanic	White
Panel A: Math performance					
CEP	0.002	0.004	-0.002	0.017	0.009
	(0.010)	(0.008)	(0.012)	(0.012)	(0.007)
Observations	65800	65800	30530	25258	51056
Baseline FRP	0.586	0.586	0.625	0.573	0.554
Baseline DV mean	-0.274	-0.274	-0.587	-0.376	-0.036
Panel B: Reading performance					
CEP	-0.012**	-0.006	-0.014	0.003	0.002
	(0.005)	(0.005)	(0.009)	(0.008)	(0.005)
Observations	68779	68779	31547	26207	52943
Baseline FRP	0.585	0.585	0.623	0.571	0.553
Baseline DV mean	-0.263	-0.263	-0.530	-0.460	0.001
Area and district controls		X	X	X	X
Sample	All	All	All	All	All

Notes: Table presents weighted least squares regression results from Equation 2.1 for all district-grade observations in which any school serving grade g participated in CEP by 2017. CEP equals one if any school serving grade g in district d participated in CEP by year t. Race/ethnic proficiency scores available for cells with at least 20 students. All specifications include district, grade, and year fixed effects. "Area and district controls" include student racial/ethnic composition and segregation, student-teacher ratios, percent of students attending a charter school, child poverty rates and county unemployment rates. Robust standard errors clustered by district. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table 2.5: Baseline District Summary Statistics by Baseline FRP Eligibility

	(1)	(2)
	Not exposed	Exposed
Panel A: Baseline area characteristics		
% FRP	0.718	0.453
	(0.111)	(0.110)
Urban	0.165	0.144
	(0.371)	(0.351)
% college educated*	0.148	0.187
	(0.074)	(0.093)
Unemployment rate	0.066	0.051
	(0.030)	(0.020)
% single mother*	0.386	0.272
	(0.134)	(0.085)
Median household income ($1000s)*	39.89	54.36
	(10.24)	(13.60)
Gini coefficient*	0.424	0.390
	(0.050)	(0.046)
Child poverty rate*	0.311	0.202
	(0.100)	(0.084)
Per-capita income assistance ($1000s)	1.291	0.996
	(0.437)	(0.311)

Table 2.5: (continued)

	(1)	(2)
	Not exposed	Exposed
Panel B: Baseline district characteristics		
% charter schools	0.037	0.027
	(0.089)	(0.069)
% black	0.343	0.106
	(0.336)	(0.152)
% Hispanic	0.197	0.137
	(0.287)	(0.224)
% special education	0.140	0.147
	(0.062)	(0.047)
# schools	16.530	16.980
	(49.410)	(34.670)
Student-teacher ratio	17.890	15.610
	(135.200)	(4.595)
Per-pupil expend ($1000s)	13.730	13.100
	(4.143)	(3.937)
School meal revenue ($1000s)	0.474	0.328
	(0.143)	(0.111)
% full CEP participation (2017)	0.695	0.566
	(0.460)	(0.496)

	(1)	(2)
	Not exposed	Exposed
Panel C: Baseline district performance		
Overall math	-0.429	-0.121
	(0.346)	(0.300)
Hispanic math	-0.437	-0.315
	(0.308)	(0.285)
White math	-0.101	0.011
	(0.346)	(0.320)
Black math	-0.641	-0.502
	(0.294)	(0.281)
Overall reading	-0.421	-0.104
	(0.320)	(0.283)
Hispanic reading	-0.528	-0.391
	(0.283)	(0.269)
White reading	-0.067	0.051
	(0.314)	(0.281)
Black reading	-0.588	-0.441
	(0.257)	(0.254)
Observations	10433	10281

Notes: Table shows baseline characteristics by year of CEP implementation for district-grades with any school participating in CEP between 2012 and 2017. “Not exposed” describes participting district-grades with baseline FRP eligibility above 57.9 percent (the median among all CEP districts); “Exposed” described districts with baseline eligibility below 57.9 percent.Baseline defined as 2006-2010 for data available through the American Community Survey (denoted by “$*$”), 2009-2011 for other sources. Column headers denote the first year a district-grade had any CEP participation. Unemployment rate from BLS LAUS, child poverty rates from Census Bureau SAIPE, per-capita income assistance from BEA REIS. Other area characteristics from the American Community Survey, and all district resources and performance measures from SEDA. All dollars in 2017 constant thousand dollars, adjusted for inflation with the CPI-U-RS. See text for details.

Table 2.5: Effect of CEP on Academic Performance: Exposed Districts Sample

	(1)	(2)	(3)	(4)	(5)	(6)
	Overall	Black	Hispanic	White	WB gap	WH gap
	Panel A: Math performance					
CEP	0.016*	0.028	0.034**	0.017*	0.020**	0.002
	(0.009)	(0.018)	(0.016)	(0.010)	(0.010)	(0.010)
Observations	32694	11658	12698	29325	11370	11465
Baseline FRP	0.454	0.457	0.438	0.458	0.457	0.449
Baseline DV mean	-0.121	-0.502	-0.315	0.0111	0.626	0.457
FRP gain	0.319	0.228	0.212	0.317	0.226	0.204
	Panel B: Reading performance					
CEP	0.007	0.016*	0.017*	0.008	0.004	0.005
	(0.006)	(0.010)	(0.010)	(0.007)	(0.010)	(0.009)
Observations	34344	12185	13256	30581	11894	11826
Baseline FRP	0.453	0.457	0.436	0.458	0.457	0.449
Baseline DV mean	-0.104	-0.441	-0.391	0.0509	0.592	0.532
FRP gain	0.319	0.230	0.209	0.317	0.228	0.200
Area and district controls	X	X	X	X	X	X
Sample	Exposed	Exposed	Exposed	Exposed	Exposed	Exposed

Notes: Table presents weighted least squares regression results from Equation 2.1 for "exposed" district-grade observations with a baseline FRP eligibility share below 57.9 percent (the baseline median among CEP districts) in which any school serving grade g participated in CEP by 2017. CEP equals one if any school serving grade g in district d participated in CEP by year t. Race/ethnic proficiency scores available for cells with at least 20 students. "FRP gain" is the share of students gaining access to free meals under CEP relative to the baseline (2009-2011) period. All specifications include district, grade, and year fixed effects, as well as student racial/ethnic composition and segregation, student-teacher ratios, percent of students attending a charter school, child poverty rates and county unemployment rates. Robust standard errors clustered by district. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table 2.6: Effects of CEP on Academic Performance in Exposed Districts: By Grade and Race

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Overall		Black		Hispanic		White	
	Math	Reading	Math	Reading	Math	Reading	Math	Reading
	Panel A: Elementary (grades 3-5)							
CEP	0.020*	0.007	0.055**	0.019*	0.050**	0.024	0.006	0.018
	(0.011)	(0.007)	(0.022)	(0.011)	(0.020)	(0.015)	(0.008)	(0.011)
Observations	17989	18419	6411	6551	7387	7457	16452	16089
Baseline FRP	0.456	0.456	0.455	0.455	0.437	0.437	0.460	0.460
Baseline DV mean	-0.082	-0.050	-0.458	-0.383	-0.288	-0.352	0.103	0.053
	Panel B: Middle (grades 6-8)							
CEP	0.006	0.004	0.003	-0.017	0.008	0.006	0.008	0.010
	(0.008)	(0.010)	(0.014)	(0.016)	(0.012)	(0.017)	(0.009)	(0.011)
Observations	15925	14705	5634	5247	5799	5311	14129	13236
Baseline FRP	0.451	0.452	0.459	0.460	0.434	0.439	0.456	0.456
Baseline DV mean	-0.166	-0.167	-0.507	-0.552	-0.439	-0.349	-0.009	-0.038
Area and district controls	X	X	X	X	X	X	X	X

Notes: Table presents weighted least squares regression results from Equation 2.1 for district-grade observations with a baseline FRP eligibility share below 57.9 percent (the baseline median among CEP districts) in which any school serving grade g participated in CEP by 2017. CEP equals one if any school serving grade g in district d participated in CEP by year t. Race/ethnic proficiency scores available for cells with at least 20 students. All specifications include district, grade, and year fixed effects, as well as student racial/ethnic composition and segregation, student-teacher ratios, percent of students attending a charter school, child poverty rates and county unemployment rates. Robust standard errors clustered by district. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table 2.7: Effect of CEP on District Resources, Exposed Districts

	(1)	(2)	(3)	(4)	(5)
	Log(fed revenue)	Log(all revenue - nutr asst)	Log(per-pupil expend)	Log(per-pupil instruct. expend)	Student-teacher ratio
CEP	0.020	0.005	0.009	-0.016**	-0.418**
	(0.012)	(0.006)	(0.011)	(0.006)	(0.212)
Observations	33356	33419	28747	28927	33419
Baseline FRP	0.453	0.453	0.453	0.453	0.453
Baseline DV mean ($1000s)	1.747	13.150	13.080	6.828	15.490
Student characteristics	X	X	X	X	X
Sample	Exposed	Exposed	Exposed	Exposed	Exposed

Notes: Table presents weighted least squares regression results from Equation 2.1 for district-grade observations with a baseline FRP eligibility share below 57.9 percent (the baseline median among CEP districts) in which any school serving grade g participated in CEP by 2017. CEP equals one if any school serving grade g in district d participated in CEP by year t. Race/ethnic proficiency scores available for cells with at least 20 students. All specifications include district, grade, and year fixed effects, as well as student racial/ethnic composition and segregation, percent of students attending a charter school, child poverty rates and county unemployment rates. All dollars in constant 2017 dollars, adjusted for inflation with the CPI-U-RS. Robust standard errors clustered by district. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table 2.8: Effect of CEP on School and District Student Composition

	(1)	(2)	(3)
	Black	Hispanic	White
	% district population		
CEP	0.001	-0.003***	0.001
	(0.001)	(0.001)	(0.001)
Observations	34454	34454	34453
Baseline DV mean	0.107	0.137	0.713
	% students CEP schools, partial participation districts		
CEP	0.009	0.003	0.003
	(0.006)	(0.004)	(0.002)
Observations	4115	4414	4872
Baseline DV mean	0.868	0.868	0.846
	Dissimilarity index, districts with multiple schools		
CEP	0.013**	0.001	0.008***
	(0.005)	(0.005)	(0.003)
Observations	26228	26228	26228
Baseline DV mean	0.280	0.256	0.249
Exposure	Exposed	Exposed	Exposed

Notes: Table presents unweighted regression results from Equation 2.1 for observations with a district baseline FRP eligibility share below 57.9 percent (the baseline median among CEP districts) in which any school serving grade g participated in CEP by 2017. CEP equals one if any school serving grade g in district d participated in CEP by year t. Panel (a) indicates changes in district composition for each race/ethnic group r; panel (b) displays the fraction of students in racial/ethnic group r attending CEP schools; panel (c) indicates the district-grade racial/ethnic dissimilarity indices following initial CEP adoption. Panels (b) and (c) are limited to districts with multiple schools, panel (b) is limited to districts with partial CEP participation. All specifications include year, grade, and district fixed effects. Robust standard errors clustered by district. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table 2.9: Effects of CEP on Math Performance, Exposure Distribution

	(1)	(2)	(3)	(4)	(5)
Baseline FRP eligible	$\leq 40\%$	$\leq 50\%$	$\leq 60\%$	$\leq 70\%$	$\leq 80\%$
	Panel A: Overall performance				
CEP	0.009	0.017*	0.017*	0.012	0.009
	(0.018)	(0.010)	(0.009)	(0.009)	(0.009)
Observations	7654	20194	35798	49444	58815
Average baseline FRP	0.315	0.402	0.466	0.516	0.552
Baseline DV mean	-0.024	-0.079	-0.133	-0.187	-0.231
	Panel B: Black performance				
CEP	0.044	0.026	0.026	0.010	0.007
	(0.038)	(0.022)	(0.017)	(0.015)	(0.012)
Observations	2728	7282	13021	20188	26192
Average baseline FRP	0.326	0.407	0.471	0.535	0.583
Baseline DV mean	-0.451	-0.476	-0.511	-0.540	-0.564
	Panel C: Hispanic performance				
CEP	0.021	0.036*	0.031**	0.017	0.019
	(0.026)	(0.020)	(0.015)	(0.013)	(0.013)
Observations	3746	8194	13920	19226	23109
Average baseline FRP	0.299	0.383	0.452	0.505	0.545
Baseline DV mean	-0.284	-0.285	-0.321	-0.341	-0.362
	Panel D: White performance				
CEP	0.012	0.017	0.018*	0.011	0.010
	(0.020)	(0.011)	(0.009)	(0.008)	(0.007)
Observations	6456	18191	32039	43066	49214
Average baseline FRP	0.326	0.409	0.469	0.515	0.543
Baseline DV mean	0.122	0.042	0.006	-0.015	-0.031
Percentile baseline FRP distribution	11.700	31.000	54.800	75.500	89.600

Notes: Table presents results from Equation 2.1 for all district-grade observations in which any school serving grade g participated in CEP by 2017 based on the baseline (2009-2011) share of students FRP eligible under the traditional formula. $CEP = 1$ if any school serving grade g in district d participated in CEP by year t. Race/ethnic proficiency scores available for cells with at least 20 students. "Average baseline FRP" indicates average baseline eligibility rates. "Percentile baseline FRP distribution" displays the share of districts with baseline eligibility $\leq x\%$. All specifications include district, grade, and year fixed effects, as well as student racial/ethnic composition and segregation, student-teacher ratios, percent of students attending a charter school, child poverty rates and county unemployment rates. Robust standard errors clustered by district. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table 2.10: Heterogeneous Effects of CEP on Math Performance, High Exposure Sample

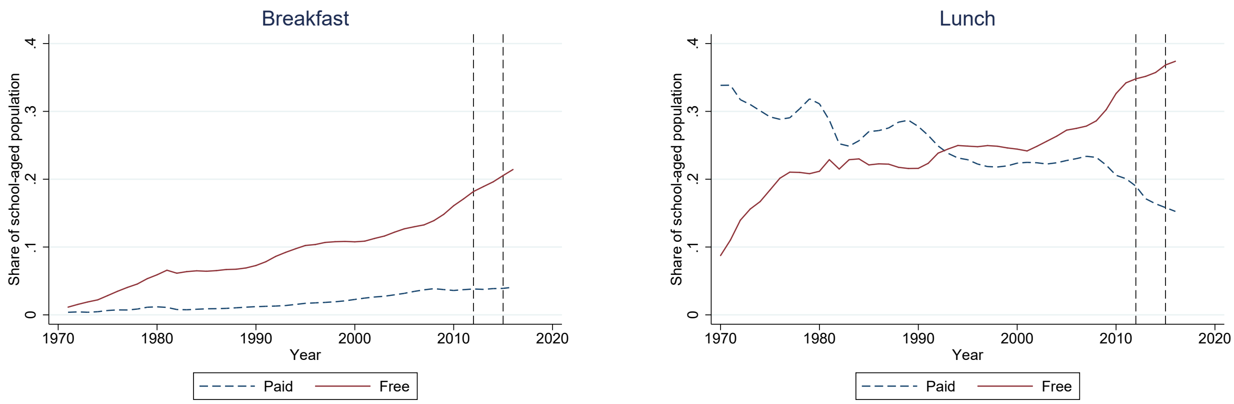
	(1)	(2)	(3)	(4)
	High cost of living	Urban	% SNAP	Per-capita Income asst
	Panel A: Overall			
CEP	0.035***	0.016*	0.019*	0.019*
	(0.011)	(0.010)	(0.010)	(0.011)
CEP X char	-0.043***	-0.001	-0.013	-0.010
	(0.013)	(0.013)	(0.014)	(0.013)
Observations	32694	32694	32694	32694
p value: CEP + CEP X char	0.480	0.265	0.631	0.436
	Panel B: Black			
CEP	0.063**	0.033*	0.034*	0.035*
	(0.027)	(0.020)	(0.021)	(0.021)
CEP X char	-0.062**	-0.013	-0.039	-0.035
	(0.029)	(0.021)	(0.031)	(0.028)
Observations	11658	11658	11658	11658
p value: CEP + CEP X char	0.925	0.353	0.846	0.999
	Panel C: Hispanic			
CEP	0.054**	0.037**	0.028	0.033*
	(0.024)	(0.017)	(0.017)	(0.017)
CEP X char	-0.026	-0.007	0.031*	0.002
	(0.023)	(0.017)	(0.018)	(0.024)
Observations	12698	12698	12698	12698
p value: CEP + CEP X char	0.101	0.142	0.004	0.163

Table 2.10: (continued)

	(1)	(2)	(3)	(4)
	High cost of living	Urban	% SNAP	Per-capita Income asst
		Panel D: White		
CEP	0.032***	0.016	0.021*	0.022*
	(0.011)	(0.010)	(0.011)	(0.011)
CEP X char	-0.038***	0.009	-0.015	-0.014
	(0.014)	(0.015)	(0.017)	(0.015)
Observations	29325	29325	29325	29325
p value: CEP + CEP X char	0.629	0.118	0.721	0.587
Area and district controls	X	X	X	X
char $\geq$	91.600		0.271	1.082

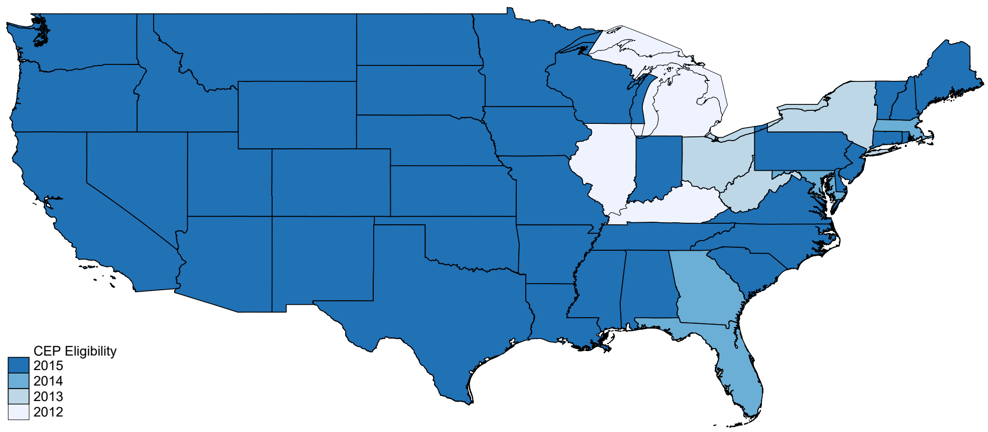
Notes: Table presents weighted least squares regression results from Equation 2.1 for district-grade observations with a baseline FRP eligibility share below 57.9 percent (the baseline median among CEP districts) in which any school serving grade g participated in CEP by 2017. Race/ethnic proficiency scores available for cells with at least 20 students. CEP equals one if any school serving grade g in district d participated in CEP by year t. $CEPXchar$ equals one for districts with the baseline characteristic provided in the column header above the median. $char \geq$ displays the cutpoint for the interaction term (e.g.: districts with a regional purchasing power of at least 91.6 are considered "high-cost" areas). All specifications include district, grade, and year fixed effects, as well as student racial/ethnic composition and segregation, student-teacher ratios, percent of students attending a charter school, child poverty rates and county unemployment rates. Robust standard errors clustered by district. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Figure 2.1: Fraction 5-17 Year-Olds Receiving School Meals by Payment Category



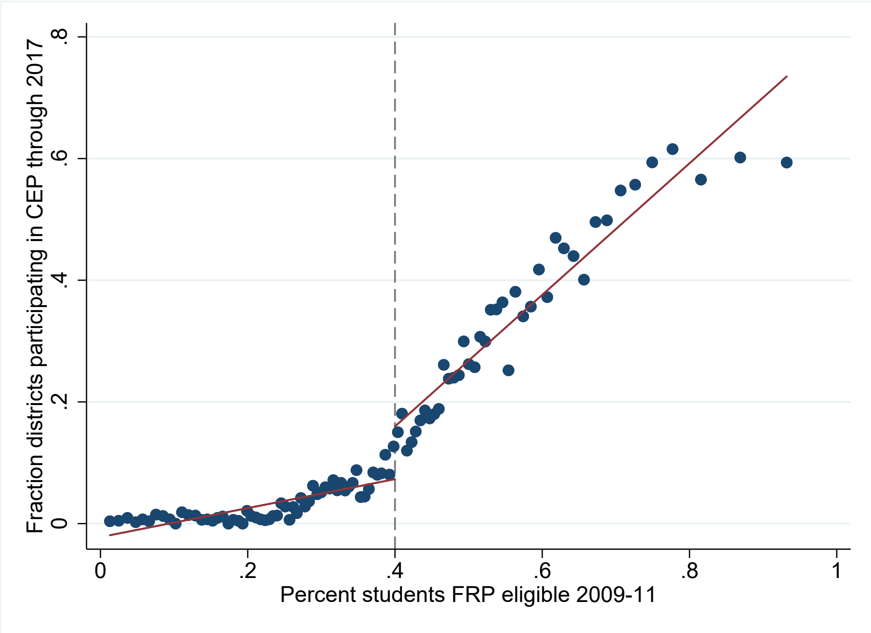
Notes: Figure shows the share of 5-17 year-olds receiving a school meal each year by payment amount. School meal counts from USDA (2018). Population estimates from Census Bureau decennial census and intercensal estimates. Left dashed vertical line denotes 2012, the year schools in the first pilot states became eligible to adopt CEP. Right dashed line denotes 2015, the first year schools in all states were eligible to adopt CEP.

Figure 2.2: First Year of CEP Availability, by State



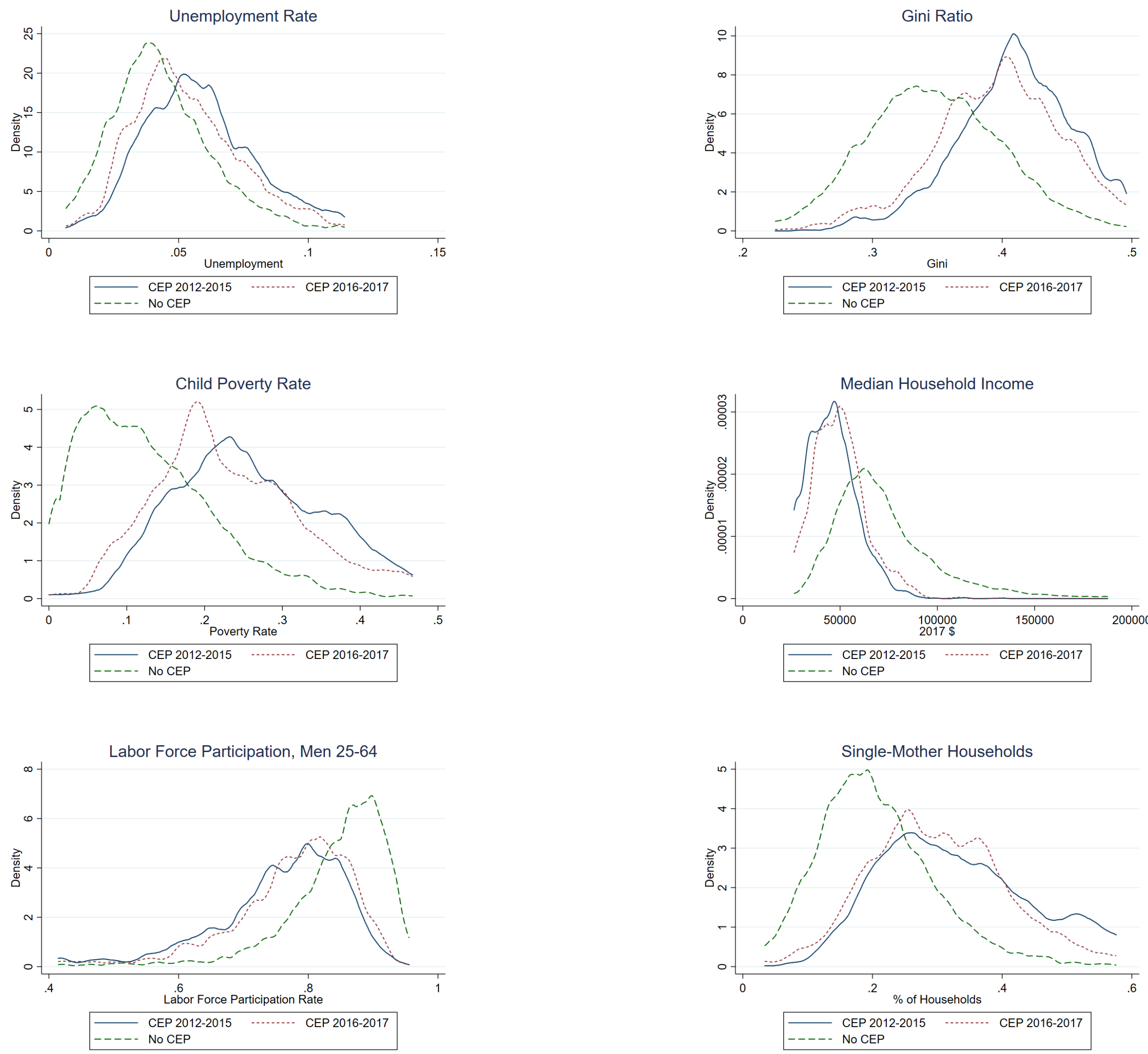
Notes: Source USDA, 2011, 2012, 2013, 2014.

Figure 2.3: CEP Participation by Baseline Share of Students Eligible for Free Meals



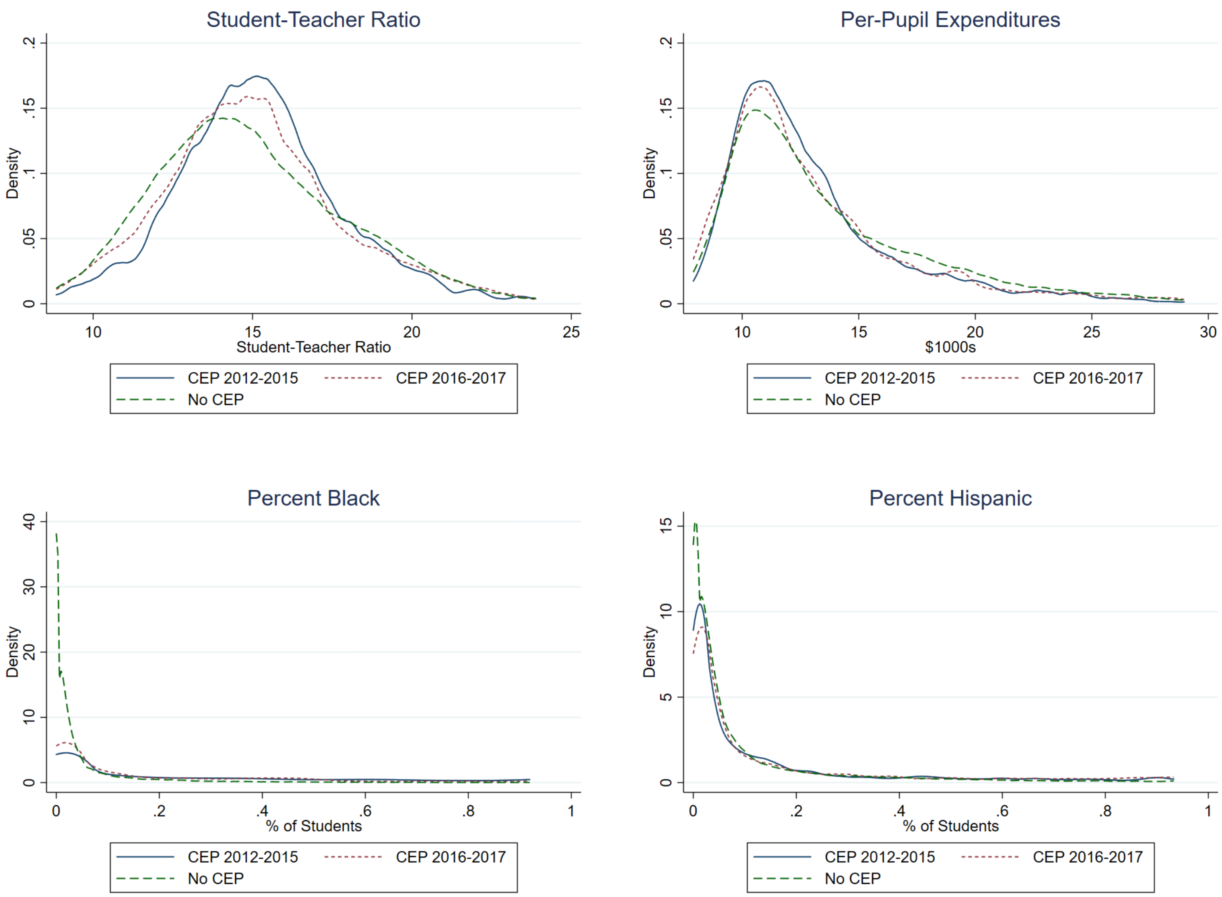
Notes: Figure plots relationship between baseline share of FRP students in a district in 2009-2011 (horizontal axis) and the probability a district participated in CEP by 2015 (vertical axis) from a binscatter of 100 equal-sized bins. The vertical line at 40 percent shows the minimum (school-level) FRP eligibility rate required for CEP participation. Sources: USDA FRAC/CBPP, Common Core of Data.

Figure 2.4: Baseline Area Economic-Well-being by CEP Adoption



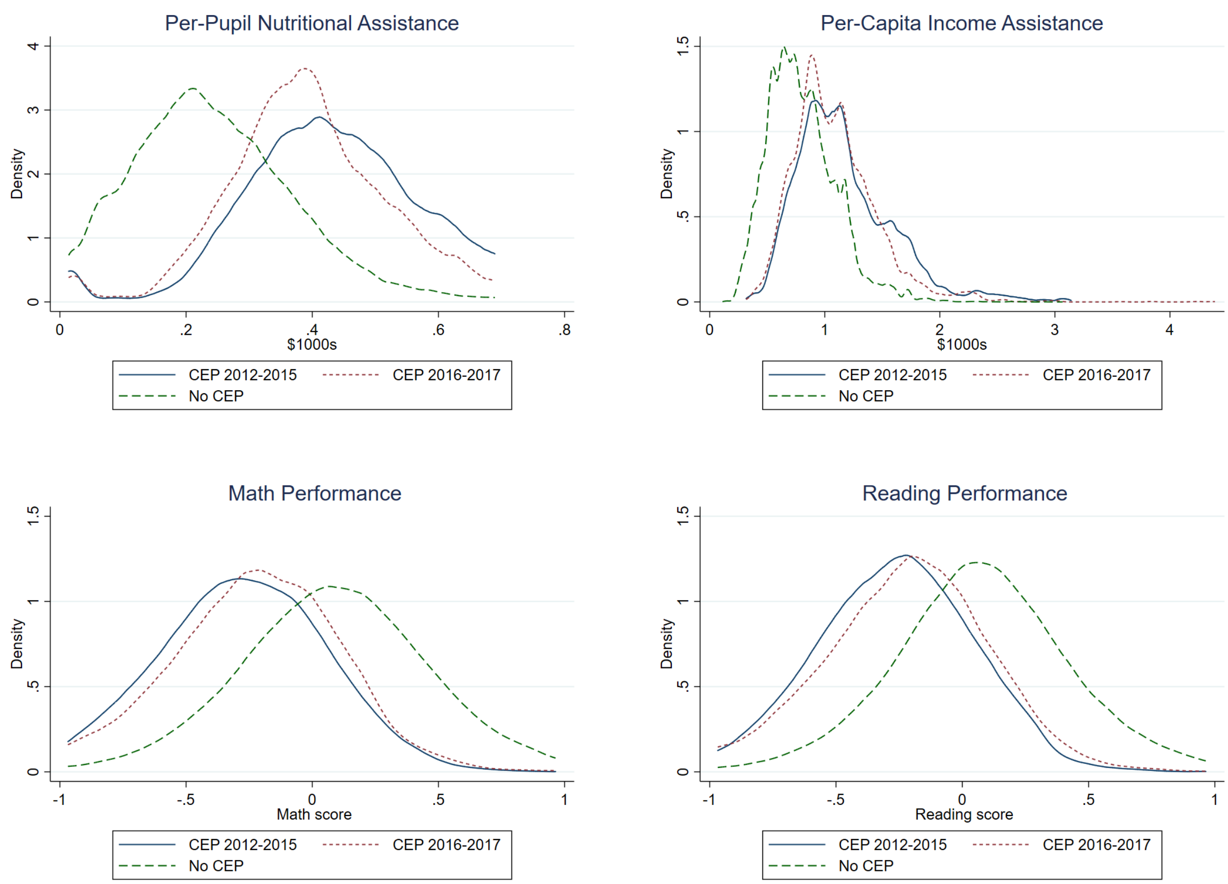
Notes: Figure shows the distribution of baseline area characteristics (2006-2010 for ACS variables, 2009-2011 for unemployment and labor force participation) by the year a district-grade first participated in CEP. "No CEP" are district-grades that did not adopt CEP by 2017. See text for variable details.

Figure 2.5: Baseline District Student Characteristics by CEP Adoption



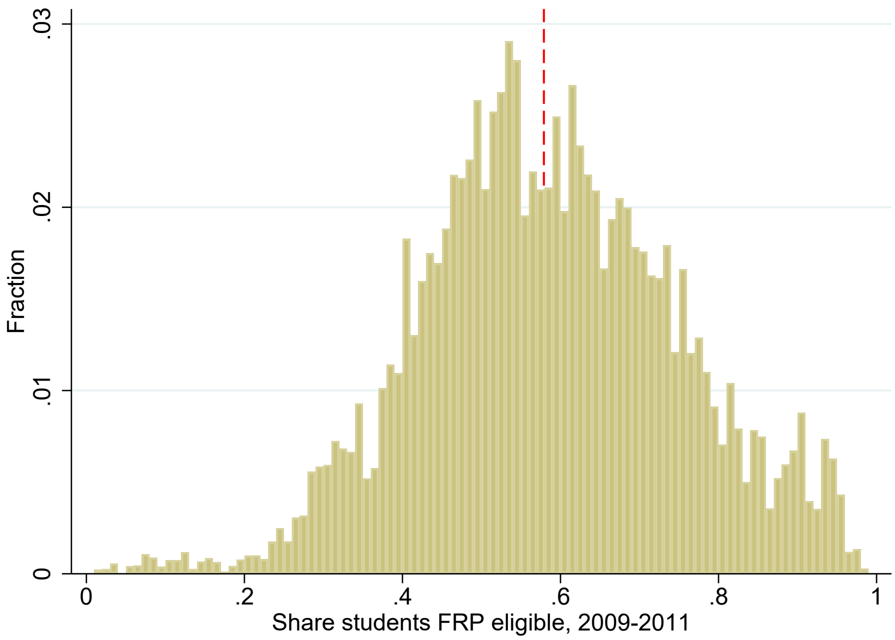
Notes: Figure shows the distribution of baseline (2009-2011) district characteristics by the year a district-grade first participated in CEP. "No CEP" are district-grades that did not adopt CEP by 2017. All dollars in constant 2017 dollars, adjusted for inflation with the CPI-U-RS. See text for variable details.

Figure 2.6: Baseline District Income Assistance and Student Baseline Performance by CEP Adoption



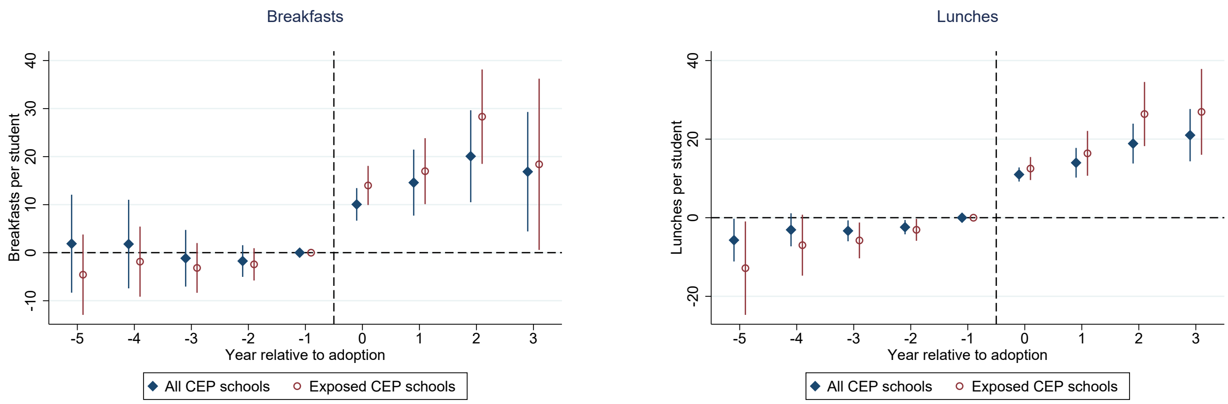
Notes: Figure shows the distribution of baseline (2009-2011) district characteristics by the year a district-grade first participated in CEP. "No CEP" are district-grades that did not adopt CEP by 2017. All dollars in constant 2017 dollars, adjusted for inflation with the CPI-U-RS. See text for variable details.

Figure 2.7: Baseline FRP Eligibility, CEP-participating Districts



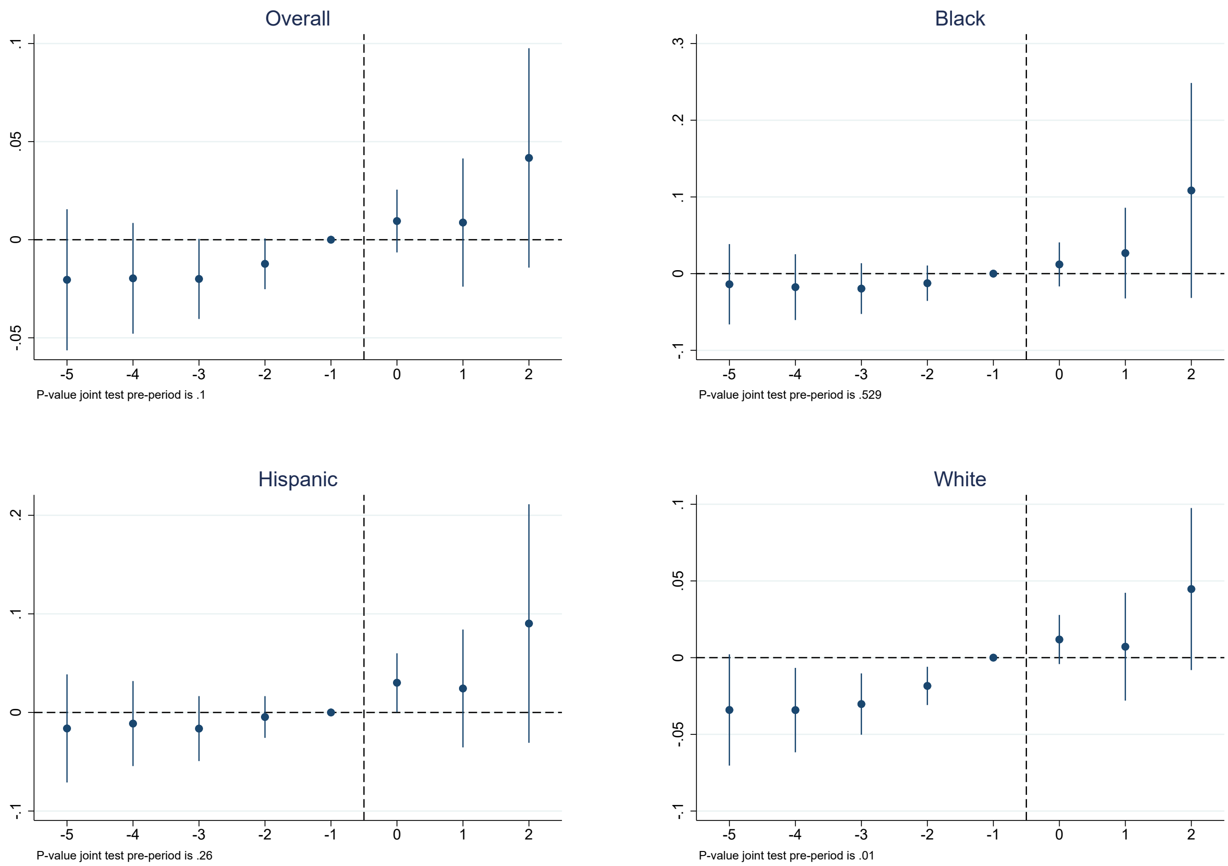
Histogram shows the distribution of the fraction of students in each CEP-participating district-grade who were eligible for free meals between 2009 and 2011. The dashed vertical line denotes the median eligibility rate among CEP-participating districts – 57.9 percent (the "exposed" cutpoint in the main analyses).

Figure 2.8: Event Study: Breakfast and Lunch Participation



Notes: Figure presents results from the (school-level) event study framework in Equation 2.2. All specifications include controls for student demographics, the fraction of charter schools in a district, child poverty and unemployment rates, and measures of racial/ethnic segregation, year fixed effects, grade fixed effects, and school fixed effects. Bars denote 95 percent confidence intervals from robust standard errors clustered by district. Exposed subsample includes schools in districts with a baseline FRP eligibility rate below 57.9 percent (the median among CEP-adopting districts). p-values for joint hypothesis that pre-period coefficients are different from zero are as follows: Breakfasts: 0.09 (all), 0.17 (exposed); Lunches: 0.02 (all), 0.01 (exposed).

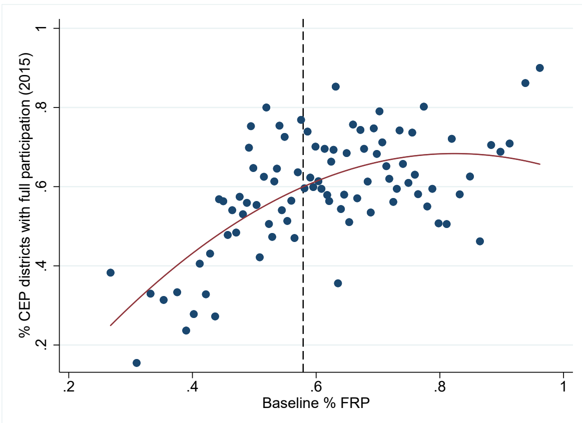
Figure 2.9: Math Performance Event Study



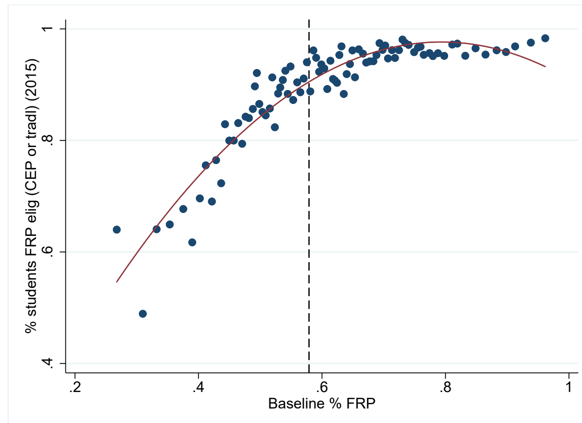
Notes: Figure presents results from the (district-level) event study framework in Equation 2.2. All specifications include controls for student demographics, the fraction of charter schools in a district, child poverty and unemployment rates, and measures of racial/ethnic segregation, year fixed effects, grade fixed effects, and district fixed effects. Bars denote 95 percent confidence intervals from robust standard errors clustered by district. Sample includes districts with a baseline FRP eligibility rate below 57.9 percent (the median among CEP-adopting districts). Notes below each panel present p-values from the joint test that pre-treatment coefficients equal to zero.

Figure 2.10: Baseline Poverty and Free Meal Access

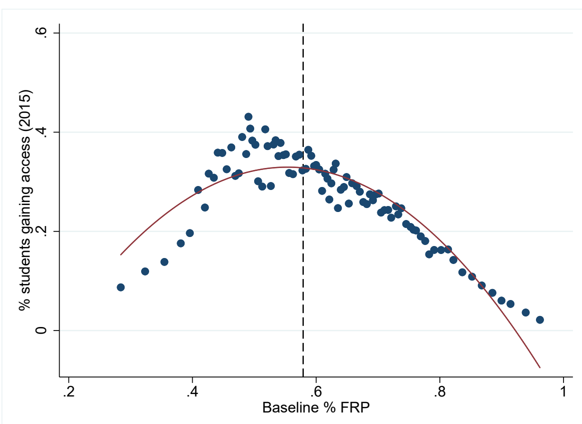
(a) Share of districts with full participation



(b) Share of students with access to free meals (either traditional or CEP)



(c) Share of students gaining access to free meals under CEP



Notes: Figure summarizes the relationship between various measures of CEP participation and access to free meals for all districts that adopted CEP by 2017. Dashed vertical line denotes the 57.9 percent threshold for the exposed subsample. Panel (a) presents the relationship between district baseline FRP eligibility and the probability of full district CEP participation; panel (b) shows the relationship between baseline FRP eligibility and the share of students with access to free meals after CEP was implemented (either students attending CEP schools or FRP students attending non-CEP schools); panel (c) shows the relationship between baseline eligibility and the fraction of students not previously eligible who gained access to free meals under CEP.

Chapter 3

Long-Term Gains from Longer School Days

3.1 Introduction

This chapter examines whether additional time in school translates into improved economic well-being in adulthood. Although policymakers frequently advocate lengthening the school day in order to promote economic growth and competitiveness, the relationship between the length of the school day and adult outcomes has not been fully explored.[1] Despite the lack of empirical evidence on this relationship, multiple Latin American and European countries have taken drastic steps to lengthen the school day over the past thirty years. In the United States, the share of kindergarteners attending full-day school increased from less than 20 percent in 1970 to 75 percent by 2012 (Gibbs, 2014). More broadly, until the 1990s, the typical student attended school for approximately four hours in many middle-income and developing countries. While some of these countries have moved towards a 6-7 hour school day, many Latin American countries continue to operate under the half-day model (UNESCO TERCE, 2016).

This chapter examines one of the first and largest full-day schooling reforms, Jornada Escolar Completa (JEC), which increased the school day for Chilean elementary and secondary school students in all publicly-funded schools by approximately 30 percent between 1997 and 2010. Due to budgetary and logistical constraints, the timing of the introduction and expansion of full-day schooling varied across both cities and birth cohorts. We leverage both sources of variation to examine the effect of additional time in school on labor market outcomes in adulthood. Specifically, we estimate the expected number of years a student would be expected to attend a full-day school using data from the Chilean Ministry of Education and match this administrative data to nationally-representative labor market data based on survey respondents' reported year and place of birth.

[1]See, for example, President Obama's March 10, 2009 speech to the Hispanic Chamber of Commerce. `http://www.nytimes.com/2009/03/10/us/politics/10text-obama.html`

This approach makes an important methodological contribution to the existing literature on full-day schooling. Most existing work examines outcomes for one birth cohort or for a single jurisdiction. In contrast, our approach allows us to identify the causal effect of more time in school under relatively weak identifying assumptions regarding the timing and acceleration of the reform. Importantly, our sample cohorts were born before the reform was announced and our treatment variable is defined based on place of birth, rather than the city in which individuals actually attend school. Therefore, our measure of access is not affected by families choosing to move to areas that implemented the reform relatively early, attending a school outside their city of residence, or selecting a school within their municipality based on full-day access. We further account for potential non-random selection into full-day schools by limiting our comparisons to birth cohorts within a geographical region and controlling for local factors that may affect both reform implementation and long-term outcomes.

Our findings are threefold. First, we find that a longer school day increases educational attainment, earnings, and the likelihood of working in a skilled occupation in adulthood. The implied earnings gains suggest a 16 percent rate of return to an additional year-equivalent of schooling, in line with existing estimates on the returns to education during the 1990s and early 2000s in Chile (OECD, 2013; Manacorda et al., 2010).

Second, we extend the analysis of Berthelon and Kruger (2011) and find access to a longer school day delays childbearing among women. In interpreting these results, we note that women in our sample are in their prime childbearing years, and we would expect the full range of intergenerational benefits to arise in the coming decades.

Third, while access to longer school days increases educational attainment for all types of students, students from higher socioeconomic groups who have greater access to full-day schools work in managerial, professional, and technical occupations, while students from disadvantaged backgrounds are more likely to enter the workforce. These patterns suggest that given students from different family backgrounds have heterogeneous labor market adaptations and disadvantaged students may face additional constraints in gaining access to skilled occupations.

Our estimates measure exposure to full-day schools, or the intent to treat (ITT) of having access to additional schooling. From a national government's perspective, this is a key parameter to determine the effect of offering additional time in school. In our setting, the ITT is equivalent to the treatment-on-the-treated (TOT) for students who attend a publicly-funded school in the municipality in which they are born. During the time our sample attended school, approximately 90 percent of students attended a publicly-funded school, and 70 percent resided in the same municipality in which they were born.[2] Among those who moved, access to full-day schooling was similar in the municipality of residence as in the city of birth, with families tending to move cities with similar access to JEC. These findings suggest the difference between the ITT and TOT is likely small for most populations

[2]For this time period, we do not have information on the municipality students attended school, which could differ from the city of residence.

in the aggregate.

This work builds upon an existing literature looking at the effectiveness of additional time in school on student outcomes. Most of the previous research examines the short-term effects of longer school days by focusing on outcomes of current students, either during large-scale reforms affecting all elementary and secondary school students, or from reforms that targeted a particular age group of students, such as the expansion of full-day kindergarten in the United States. The findings on how large-scale reforms affect academic outcomes are mixed, with some studies finding no significant effect and others finding modest positive effects on students' test scores (Bellei, 2009; Valenzuela, 2005; García, 2006; Dias Mendes, 2011; Llambí, 2013; Orkin, 2013).

Extended school days may affect outcomes other than academic performance. For young children, longer school days provide a form of subsidized childcare and may therefore increase parental employment and family income, particularly for families with young students (Berthelon et al., 2015; Contreras et al., 2010; Gibbs, 2014; Gelbach, 2002). For younger students, there is evidence of medium-term benefits, as students accessing longer days in kindergarten have higher educational attainment and lower involvement with the criminal justice system (Cascio, 2009). Older students also appear to benefit, but from a more direct, "incapacitation" mechanism. For teenagers, additional time in school may reduce risky behaviors that occur outside of school, such as teen pregnancy and involvement with the criminal justice system (Berthelon and Kruger, 2011; Contreras et al., 2010). This chapter contributes to previous work by evaluating the *long-run* effects of additional school time once students have completed schooling and entered the labor market.

As we measure the long-term effects of a policy that changed students' time use patterns and family resources, there are several potential mechanisms for our findings. Like all evaluations of full-day schooling reforms, we are unable to conclusively determine whether these findings are due to increased human capital attainment through additional instruction time, access to newer school facilities, changing students' time use patterns, or a combination of factors, as infrastructure changes in the education environment coincide with the expansion of the school day. Second, it is important to note that children who gain access to longer school days in early grades are also more likely to have access in later grades. We are therefore unable to fully distinguish the extent to which these findings are driven by dynamic complementarities – in which additional learning at young ages facilitates future knowledge accumulation – or simply additional schooling at any point during one's academic career. Despite these limitations, we can rule out some other possible channels for earnings gains. In particular, we do not find longer school days change migration patterns across municipalities within Chile. In addition, most of our main effects are somewhat smaller for individuals born in areas experiencing the largest increases in maternal labor supply over the reform period, inconsistent with a scenario where schooling benefits students *exclusively* through a family income subsidy.

This chapter proceeds as follows. Section 3.2 discusses the existing literature on school day length and well-being. Section 3.3 describes the Chilean reform. Section 3.4 outlines the empirical approach and data. Section 3.5 presents results, and Section 3.6 concludes.

3.2 School day length and student outcomes

The time students spend in school, measured by either hours or days, depends on both individual and area characteristics that are correlated with student outcomes (Patall et al., 2010). In addition, term length is typically strongly correlated with other quality dimensions, making it difficult to separate different aspects of educational quality (Card, 1992; Ganimian and Murnane, 2016). Both of these points caution drawing causal conclusions from simple correlations between student outcomes and the time spent in school. Perhaps unsurprisingly, early cross-sectional analyses find little association between the length of the school year and earnings in adulthood (Card, 1992; Heckman et al., 1995).

Time in school and academic achievement

A large literature examines the nature of short-term benefits from more time in school. This work focuses on current students and generally finds that longer school days improve academic performance. If improved achievement translates into greater educational attainment or human capital accumulation, these studies suggest longer school days may also improve long-term outcomes as students enter the labor force.

Several recent papers focusing on developed countries use exogenous variation in the length of the school day or year and find that additional time in school improves academic performance in the short run. Using variation in the length of the school year stemming from snow days, Goodman (2014) finds that shorter school years due to building closures do not affect performance, but individual absences due to bad weather worsen math performance. Gibbs (2014) examines the effect of full-day classes for young students and finds full-day kindergarten improves test scores 0.3 standard deviations, with particularly large gains for Hispanic students. Pischke (2007) examines a reconfiguration of the West German academic calendar when several states implemented multiple short school years in order to align with federal requirements, and finds that shorter school years worsened academic performance and increased grade repetition, but had no long-term effect on employment or earnings when students were in their 20s and 30s. Finally, within-student variation in subject-specific instructional time can measure subject-specific returns to greater educational attention. Using cross-country PISA score data, Lavy (2015) finds an additional hour of instructional time in a given subject improves scores in that subject by 0.07-0.15 standard deviations, with a weaker relationship for students in lower-income countries.

A related literature examines the short-term effects of larger-scale reforms that gradually transition *all* elementary and secondary students from a part-day to full-day schedule. Since the 1990s, a number of Latin American countries and cities have expanded the school day from approximately four to 6-7 hours. Chile was one of the first countries to undertake such an expansion; therefore, much of the existing literature examines the Chilean reform.

The results from these reforms are mixed across countries, but generally point towards improved school performance (Glewwe et al., 2013). While some studies find worsened test scores following reforms targeted to disadvantaged or low-performing schools in Brazil

(Dias Mendes, 2011) and Uruguay (Llambí, 2013), other studies find improvements in Colombia (Hincapie, 2016; Bonilla, 2011) and Buenos Aires (Llach et al., 2009). In comparing the magnitudes across different types of reforms, it is important to note that larger-scale full-day reforms are more likely to change other school resources – such as requiring new facilities, offering mid-day school meals, and providing additional recess time. Since these changes to the school day and educational system occur concurrently, examinations of large-scale full-day reforms– including the current study–cannot separate the effect of greater time in school from other concurrent changes to the learning environment. As summarized by Ganimian and Murnane (2016), this literature finds that resources need to change students' experiences, that is – by improving the quality or quantity of instructional time, or improving nutritional intake, rather than offering "more of the same" – in order to affect achievement.

The Chilean reform is an example of a reform that extended the school day and allocated most of the additional time to greater instruction and increased teaching resources (DESUC, 2005; Barrios and Bovini, 2019).[3] Previous work has documented that the reform improved academic performance by 0.05-0.20 standard deviations (Bellei, 2009; Valenzuela, 2005; García, 2006), and accounting for non-random selection into full-day schools yields larger estimated improvements than the unadjusted results (Berthelon et al., 2016).

Given the findings of the previous literature, we take the possibility of non-random selection into full-day schools seriously and estimate an intent-to-treat effect of having access to full-day schools. Our "access" measure is determined by factors that pre-date the reform announcement – namely city of birth – and is not affected by subsequent migration patterns. This approach is similar in spirit to Berthelon et al. (2016) who instrument full-day school enrollment using municipality rates of full-day school exposure the year before students' outcomes are measured. The main differences are that our approach aggregates access to full-day schooling over an individual's entire academic career in order to provide a measure of exposure over an individual's full career and as our data lack information on which schools a student actually attends, our approach is a reduced-form strategy based on a student's city of birth that does not allow us to precisely estimate the magnitude of endogenous mobility.

Time in school and outcomes in adolescence and early adulthood

Improvements in short-term academic performance are neither a necessary nor sufficient condition for additional school time to benefit students into adulthood. If test score gains fade over time, or if skills that are measured by standardized tests are not correlated with labor market productivity, short-term improvements may not lead to labor market advantages. As noted in Murnane et al. (2000), there is no empirical consensus on this point – some studies find a negligible relationship between short-term academic improvements and longer-term labor market outcomes in the context of full-day school reforms (Pischke, 2007; Pires and Urzua, 2015), while others find general improvements test scores are associated with higher

[3]Comprehensive information on school resources, including funding and other staff metrics is not available for the full implementation period, limiting our ability to determine the extent to which our findings are driven by increased financial or staffing resources.

earnings in adulthood (Rose, 2006; Chetty et al., 2011b; Hansen et al., 2004; Murnane et al., 1995). More broadly, there is a growing body of literature documenting that educational investments that exhibit short-term gains that fade out over the medium-term, such as classroom sizes and early childhood education, can still generate meaningful improvements in long-term economic outcomes (Chetty et al., 2011a; Garces et al., 2002).

Even if full-day schooling does not affect academic performance, there are several factors that may shape students' economic opportunities by changing families' and students' time use patterns. Extended school days provide families with an implicit childcare subsidy and reduce children's leisure time. Contreras et al. (2010) and Berthelon et al. (2016) find that Chilean full-day schools increased female labor participation and employment. Berthelon and Kruger (2011) analyze the extent to which more time in school "incapacitates" high school students from engaging with the criminal justice system or becoming a teenage mother. They find greater access to full-day schools lowers adolescent crime and reduces teenage pregnancy rates for lower-income girls in urban areas. As teen parenthood and a criminal history are associated with lower earnings later in life and higher maternal employment potentially increases family resources, all of these findings suggest channels by which additional time in school may lead to longer-term benefits. In the US context, Cascio (2009) finds that the introduction of public kindergarten increased high school graduation and reduced involvement with the criminal justice system.

The existing work on how extended time in school affects labor market outcomes is relatively limited, as in many contexts, students who attended school for a full-day are still completing their schooling or only recently entered the labor market. As Chile was one of the first countries to adopt a full-day schooling reform for elementary and secondary students, much of the work on early labor market outcomes focuses on the JEC reform. Closely related to this chapter, Pires and Urzua (2015) examine the medium-term effects of the Chilean reform by comparing students who attended full-day school starting at ages 14-15 (and were surveyed at ages 25-26 years old) to older cohorts who completed school prior to reform (29-30 years old). They find while attending full-day school improved academic performance, it only increased monthly wages among students who had previously attended the afternoon shift. There are several limitations of Pires and Urzua (2015) which the current study aims to address more fully. First, their treatment cohorts attended full-day schools for up to 3-4 years, less than half of the full treatment. In contrast, our sample covers the full implementation and provides labor force information for students with access to up to the entire 12 years of full-day schooling. Second, although insufficient time has elapsed to investigate the full earnings-age profile, we are able to extend the analysis a decade and examine employment outcomes into treated students' late 30s. A greater age range is particularly important since longer school days increase secondary or tertiary educational attainment and delay labor market entry. Finally, Pires and Urzua (2015) leverage variation in schools students actually attended and control for observable factors that may affect school choice. We take a complementary approach, by using both cross-city and cross-cohort variation that is exogenous to the choice set students face.

3.3 Full-day school reform: Jornada Escolar Completa (JEC)

Until the late 1990s, Chilean elementary and secondary students attended school 4-5 hours a day. Under this model, many schools operated a two-shift system where some students attended school in the morning (8am to 1pm), and others attended in the same building during the afternoon (2pm to 7pm). Beginning in 1997, Chile implemented Jornada Escolar Completa (JEC), a large-scale reform that increased the school day in publicly-funded schools by an average of 1.4 hours, while keeping the total number of school days fixed.[4]

JEC gradually moved all schools to a single, full-day shift, with all students attending in the morning through mid-afternoon (8am to 3pm). This reform represents a substantial increase in schooling time: on average, instructional time increased 30 percent and the total length of the school day increased 22 percent (Berthelon and Kruger, 2011). This additional time could be used for either instructional or extra-curricular activities; the stated goal was to improve school quality (Alfaro and Holland, 2012). With the available data, we are unable to observe school-level changes in instructional time and therefore cannot speak to the relative productivity of additional instruction, versus extra lunch or recreation time. In the aggregate, however, most teachers, parents, and students reported that at least some of the additional time was used for language and math instruction, while only 2 percent of respondents dedicating additional time to study for standardized tests (DESUC, 2005).

While schools could choose when to begin offering an extended school day, the reform required a substantial infrastructure investment in many areas, as building and staffing resource needs nearly doubled in areas previously operating a double shift. Due to these practical considerations, schools operating under capacity were the first schools to adopt JEC (Bellei, 2009). For schools without excess capacity, the Ministry of Education prioritized funding schools in disadvantaged areas and partially offset operational costs with a 20-50 percent increase in central government funding.[5] The legislation required that all schools receiving public funding operate a full-day schedule by 2007 (public schools) or 2010 (publicly-funded voucher schools), leading to a 14-year rollout period.[6]

[4]About 93 percent of students in the K-12 system enroll in publicly-funded schools. These schools include public schools that are managed by local municipalities and private-voucher schools that are managed by private entities but subject to central government legislation. Originally, schools were mandated to expand school day in grades 3 to 12 by 2007, but in practice most implemented the reform in grades 1 and 2. As young students also had access to longer days, we measure JEC exposure across the full 1-12 grade range. See law description at: `https://www.leychile.cl/Navegar?idNorma=76753`.

[5]In practice, JEC funds were allocated based on low switch costs and high pre-existing deficits in infrastructure. These schools tended to be relatively small and more rural (Berthelon and Kruger, 2011; Contreras et al., 2010). The exact increase in per-student revenue was school-grade-specific and depended on the grade served and other school characteristics.

[6]The reform covers public schools that are locally managed and fully funded by the central government and private subsidized voucher schools that are privately managed by receive government funds. According to administrative data from the Ministry of Education, approximately 36 percent of students attended a public school and 55 percent attended a voucher school in 2016. Private non-voucher schools were not covered by

Not all grades within a school were required to implement JEC at the same time. The school day was lengthened at the beginning of the academic year. The youngest students typically gained access relatively early in the reform and continued receiving full-day schooling as they progressed through school. Accordingly, JEC led to variation across cohorts and municipalities in access to full-day schools. Whereas 20 percent of students attended a full-day school in 1997, this fraction had skyrocketed to more than 80 percent in 2015. Since JEC was a grade-school specific change that was generally first introduced for younger students, the vast majority – over 90 percent – of students experience increased access over their educational career. Based on the level of exposure at the municipality level, defined below, we observe that only eight percent of students experience a reduction in access when moving from one grade to the next, and these changes typically occurred at the transition between primary and secondary school. Accordingly, like other work on full-day reforms, we are unable to disentangle dynamic complementarities, by which additional schooling at particular grades has especially pronounced effects, from treatment "dosage" years of full-day schooling.

We calculate exposure to JEC, $(\widehat{JEC_{cm}})$ as the expected number of years an individual born in cohort c in municipality m would attend a full-day school in grades 1 through 12 using administrative enrollment and JEC participation data from the Ministry of Education on total enrollment N for each grade g in school s serving grade g in municipality m:

$$(\widehat{JEC_{cm}}) = \frac{1}{N_{cm}} \sum_{s \in m} \sum_{g=1}^{12} \mathbb{1}\left\{JEC_{scgm}\right\} * N_{sgcm} \tag{3.1}$$

$\mathbb{1}\left\{JEC_{scgm}\right\}$ is an indicator function equal to one if school s in municipality m had implemented JEC for grade g when cohort c was in grade g. N_{sgcm} is the number of students enrolled in grade g in school s in municipality m, obtained from the Ministry of Education administrative data. We use administrative enrollment data by school-grade from the 2013 school year as it is adequately late in the implementation process to provide a measure of capacity in schools that were newly built because of JEC. Moreover, since 2013 follows the formal implementation period, enrollment in this year is less prone to intra-municipality sorting between schools that offer JEC and those that do not.[7]

As this measure of access does not depend on the school a student actually attends, but is based on students' locations before the policy was announced, it is not biased by

JEC and the share of students attending a private non-voucher school is small (about 6 percent) and remained relatively unchanged over the period when our sample was in school. In practice, the implementation period was delayed and by 2010, only about 75 percent of schools had a full-day schedule.

[7]From a practical perspective, 2013 is the earliest year enrollment data at the grade-level is available from the Ministry of Education. If schools that adopted JEC relatively early experienced increases in enrollment relative to those that adopted later within the same municipality, using a later year will lead to our estimated $(\widehat{JEC_{cm}})$ to be larger than the true $\mathbb{E}(JEC_{cm})$, and therefore our results will represent a lower bound on the returns to full-day access. We also exclude all schools with a single student in grade g in 2013. Fewer than 7 percent of school-grade observations are dropped with this restriction, and results are robust to including the full universe of schools.

students selecting in to full-day schools or moving to cities with greater JEC availability. This treatment measure is similar to approaches other work examining the effect of JEC access on contemporaneous outcomes (Berthelon and Kruger, 2011; Berthelon et al., 2016), but builds upon the point-in-time estimate by summing full-day exposure across grades 1-12 in order to obtain the total number of years a student would be expected to attend a full-day school throughout his or her career.[8]

$\widehat{JEC_{cm}}$ provides a continuous measure of full-day school access rather than a discrete measure in order to be consistent with the Chilean school setting and the nature of the reform. First, multiple schools serve a single grade in nearly all municipalities (97 percent). Accordingly, the probability a student attends a full-day school in any given year is not exactly equal to 0 or 1. Moreover, students with access to full-day schooling in an early grade may lose access in their academic careers. This pattern appears to be most common in areas where a large share of elementary schools adopted full-day schooling relatively early in the roll-out period, but secondary schools adopted relatively late.[9]

There is substantial variation in access to full-day schooling both within and across birth cohorts, shown in Figure 3.1. Panel (a) shows the 5th, 50th, and 95th percentile of JEC access by birth cohort. This figure indicates that an individual expected to attend 4 years of full-day school was in the 95th percentile of the 1986 birth cohort, the median of the 1989 cohort, and the 5th percentile of the 1993 birth cohort. Panel (b) plots the fraction of students attending a JEC school each year by municipality and illustrates substantial cross-city variation in both the introduction and expansion of full-day schools, with the thick line denoting the national average. This plot shows that some areas made a lot of initial progress in implementing JEC, but lagged in expanding to all grades, while others started slowly but quickly accelerated coverage. While less-populated areas tended to be able to expand more quickly, other area characteristics are not significantly predictive of the pace of implementation.

Figure 3.2 summarizes how this varying exposure translates into the JEC exposure distribution for our main sample. About 11 percent of our sample had no access to full-day schools; we exclude this large spike from the figure. Among the remaining 89 percent with some exposure to the reform, a quarter of those are expected to attend a full-day school for at least four years, and nine percent are expected to attend full-day schools for at least six years.

[8]The CASEN household survey only includes information on respondents' year of birth (not month-day). The Chilean school year begins in March, and children who turn five through June are eligible to enroll (McEwan and Shapiro (2008) provide a full description on Chilean enrollment cutoffs). We define age in first grade based on a child's year of birth plus six; accordingly, for children born in January through June, our approach assigns them the JEC exposure of an younger cohort (e.g.: weakly greater years of full-day schooling than they actually had access to). As our estimates err on the side of under-estimating JEC exposure, our results are a lower-bound on the actual exposure effect.

[9]As cumulative access depends on both when a cohort first gained access to JEC and how quickly the reform was expanded, a typical event study framework is not feasible in this setting. In an event study spirit, however, Figures 3.3-3.5 and 3.7-3.8 illustrate the extent to which there are constant returns to an additional year of JEC access.

3.4 Empirical approach

Data

Using an individual's year and city of birth, we map expected years of full-day schooling to data on economic outcomes in adulthood from the 2006 through 2017 waves of Chile's biennial demographic survey, the National Socioeconomic Characterization Survey (CASEN). Similar to other household surveys, such as the Current Population Survey (CPS) in the US, the CASEN is a large, regionally- and nationally-representative household survey that provides comprehensive individual-level information on labor market participation, household structure, educational attainment, family background, and income.[10] Important for our purposes, starting with the 2006 survey, each individual was asked where his or her mother was living when he or she was born, whether the current city of residence or a different city (and if the latter, which city). We use this information to identify the city of birth, linking approximately 98 percent of respondents to a birthplace. Most of the unmatched observations result from respondents reporting mother's residence at birth at a higher level of aggregation than the municipality (e.g.: the region or the province).

We limit the sample to individuals born between 1979 and 1992–those who were school-aged (between ages 5 and 18) the first year of the reform and were thus exposed to between zero and twelve years of full-day schooling. Our main sample limits the data to CASEN respondents between the ages of age 23 to 38 in order to explore how access to longer school days during childhood affects outcomes in adulthood.[11]

Table 3.1 displays summary statistics for main sample, as well as subpopulations disaggregated by gender and socioeconomic status, where socioeconomic status based on maternal educational attainment.[12] The average respondent is about 28 years old, and expected to attend full-day school for 2.0 years. These characteristics are similar by gender and family socioeconomic status. Overall, about eighty percent graduated high school, 20 percent have at least a four-year university degree, and students from less-disadvantaged backgrounds have greater levels of educational attainment. About two-thirds of the full sample worked in the previous month, and women have substantially lower participation rates than men. Even though our sample is relatively young, nearly 60 percent have children.

Appendix Table C.1 explores whether exposure to JEC is associated with student characteristics by regressing student characteristics (defined at birth) on access to JEC, controlling for survey year, municipality of birth, and region-specific cohort factors: y_{icmt} =

[10] Our main specifications use regionally-representative weights in order to provide the most comprehensive coverage of the population; results are qualitatively unchanged when using municipal- ("comuna") level weights or without weighting.

[11] For results focusing on high school graduation, we extend the sample to include individuals ages 19-22 who were born in the 1979-1992 window. Results are robust to excluding these individuals.

[12] Individuals whose mother graduated high school are considered "high SES," and those whose mother did not complete high school are defined as "low SES". Respondents with unknown maternal educational attainment are not included in either subgroup (about 20 percent of the sample from CASEN waves 2006-2015 and 40 percent for 2017).

$\alpha + \beta JEC_{icmt} + \delta_{cr} + \phi_t + \psi_m + \varepsilon_{icmt}$. There are no significant differences in access by maternal educational attainment, race, or gender. Nonetheless, all of our results control for these characteristics in order to improve precision. We also report results separately for men, women, and by maternal educational attainment in order to explore whether the aggregate results are driven by the experiences of a subpopulation.

Exposure to JEC

The Chilean JEC reform is typical of full-day reforms in other Latin American countries. Longer school days require a substantial increase in facilities and instructional resources. At the extreme, if a single building operated two school "shifts" at capacity before the reform, the transition to a full-day school would require a doubling in building space and teaching staff. Since new facilities must be built and additional teachers and staff recruited, full-day schooling reforms are typically implemented over multiple years.

One approach to estimate the effects of a longer school day would be to assume the timing of introducing a longer school day is randomly assigned and estimate the difference between students with different levels of treatment. In its most basic form, this approach would estimate the effect on outcome y of attending a full-day school for JEC_{icmt} years for individual i living in municipality m, in birth cohort c and surveyed at time t as:

$$y_{icmt} = \alpha + \beta JEC_{icmt} + \varepsilon_{icmt} \tag{3.2}$$

This simple framework requires that the introduction of JEC is uncorrelated with students' potential outcomes. There are several reasons why this assumption may not hold, even after accounting for cohort- or city-specific factors. First, Chile adopted full-day schooling during a period of robust economic growth; therefore comparing outcomes of younger to older cohorts will conflate the effect of schooling with aggregate wage growth and other improvements in economic opportunities.

Second, examining the effect of full-day schools using a single cohort and only relying on geographic variation in full-day access is also potentially problematic. Given the funding requirements of a large-scale expansion, policymakers might prioritize initial funding to undersubscribed schools or those with excess capacity. Alternatively, officials with limited resources may target early adoption to areas that better able to implement the program, or maximize the effect of the funds by targeting the neediest areas. If disadvantaged areas pilot the program, a naive OLS approach comparing early- and late-adopting schools understates any benefits. On the other hand, if these schools are located in areas better situated to support a large scale expansion, the basic framework will overstate any benefits of moving to a full-day schedule. We explore these patterns in two analyses discussed below and find that more rural areas tended to implement JEC earlier and more quickly. In order to account for these patterns, we follow the existing literature and only consider variation across cohorts within a given region and include a vector of controls for both contemporaneous economic conditions, as well as survey year trends in baseline (1996) poverty and employment rates in an individual's city of birth.

A final threat to the basic OLS design is even if JEC implementation was randomly allocated across schools over time, the within-cohort approach does not fully account for selection into full-day schools as Chilean families can choose the school their child attends, including schools outside their city of residence. Selection into full-day schools can arise from families moving across municipalities or selecting a school outside their city of residence.

We estimate that about 20 percent of school-aged children live in a different municipality than where they were born, and more disadvantaged SES populations are significantly less likely to move than students from more educated families. In our sample, most moves are local: less than 10 percent of children live in a different region than where they were born. As families that move tend to migrate to areas with nearly identical JEC access (on average, about 8 percent additional JEC among the moving sample, with a median of about 0.06 percent greater access), and we lack information on where adult CASEN respondents lived during childhood (or in which municipality they attended school), we define access to full-day schooling based on municipality of birth.

In practice, most students attend a nearby school: 95 percent travel less than 6 kilometers between home and school, and most elementary students attend a school within 2 kilometers (Gallego and Hernando, 2010; Chumacero and Paredes, 2008). Even though average travel distances are short, the nature of school selection suggests that those who enroll in full-time schools are likely those who benefit the most from the additional school time (Berthelon et al., 2016). From a practical perspective, our data do not include the exact school an individual attended.

To overcome both potential selection bias and data limitations, we measure full-day school access as "exposure" to JEC – the expected number of years a student attends a full-day school based on his or her birth city and cohort, described in Equation 3.1. Our main specifications omit the Santiago metropolitan region, as municipalities in Santiago are more geographically-proximate so students in Santiago are able to easily access a school other than their neighborhood school (Chumacero et al., 2011). We verify these patterns using attendance data from 2015 and find that about 15 percent of elementary school students in Santiago attend a school in a municipality different from where they live, compared to less than 10 percent in other regions.[13]

Importantly, the gradual rollout of JEC provides two sources of variation: first, children born in the same city are exposed to different amounts of full-day schooling based on the year they were born. Second, children born in the same year are exposed to different amounts of schooling depending on their city of birth.[14] We leverage both sources, comparing outcomes

[13]As shown in the final two columns Appendix Tables C.3, C.5, C.6, C.7, C.10, C.11, and C.9, including Santiago renders results smaller in magnitude and less precise. With the available data, we are unable to determine whether this patterns is due to a weak first stage (calculated access being a noisy measure of actual access) or heterogeneity in benefits between urban and rural locations.

[14]All individuals in our sample were born in 1992 or earlier, before the reform was announced (in 1997). As we rely on location decisions before the policy was announced (e.g.: at birth), our estimated access to

based on within- and across-cohort variation. A causal interpretation of our results therefore involves the identifying assumption that the *pace* of JEC implementation is uncorrelated with potential outcomes among students born in the same city in different years. Using the measure of JEC access $\widehat{JEC_{cm}}$ from Equation 3.1, we estimate the effect of full-day schools on outcome y_{icmt} as:

$$y_{icmt} = \beta(\widehat{JEC_{cm}}) + X'_{icmt}\gamma + Z'_{mt}\theta + \delta_{cr} + \phi_t + \alpha_t cZ_{1996r} + \psi_m + \varepsilon_{icmt} \qquad (3.3)$$

Where i indexes individuals in cohort c, born in municipality m and surveyed in year t. In order to improve precision, we include X_{icmt}, a vector of individual demographic characteristics, including age, gender, indigenous status, and maternal education. For labor market outcomes other than educational attainment and childbearing, we also include controls for marital status and number and presence of children interacted with gender and household size. We also include a vector of city characteristics for a respondent's current location, Z_{mt}, including employment and poverty rates and average earned income, as well as $\alpha_t cZ_{1996r}$, a separate survey year linear trend in baseline (1996) municipal employment and poverty rates.[15]

The empirical approach in Equation 3.3 assumes a linear treatment effect – that is, that marginal benefits are constant for each additional year of JEC exposure. In order to explore the presence of increasing or decreasing marginal returns, we adopt a less parametric approach by replacing the continuous measure of full-day exposure with nine one-year exposure bins, pooling all observations with at least eight years of exposure:

$$y_{icmt} = \sum_{y=0}^{8} \beta_y \mathbb{1}\{(\widehat{JEC_{cm}}) \in [y, y+1)\} + X'_{icmt}\gamma + Z'_{mt}\theta + \delta_{cr} + \phi_t + \alpha_t cZ_{1996r} + \psi_m + \varepsilon_{icmt} \quad (3.4)$$

When interpreting these results and reconciling with the difference-in-difference estimates, we emphasize that access to full-day schooling is heavily skewed: 30 percent of our sample has access to one year of JEC or less, and one percent is estimated to receive more than eight years of full-day instruction. With this distribution in mind, the more flexible strategy suggests diminishing marginal benefits to each additional year of full-day schooling.

The JEC implementation window covers a time period of marked improvement in economic conditions in Chile. In particular, real GDP increased about 50 percent between when the oldest and youngest individuals in our sample were born (World Bank, 2017). In addition, secondary school became mandatory for cohorts graduating in 2003 or later (Ley 19876, 2003).[16] All of our estimates include survey year fixed effects, ϕ_t to account for level differences in economic performance at the time of the survey, as well as municipality fixed effects ψ_m to control for local time-invariant characteristics. We finally include region-specific

full-day schooling is not affected by any migration decisions occurring after the policy announcement.

[15] Appendix tables show results are nearly identical when omitting baseline trends.

[16] In a series of robustness checks, we have verified the results robust to limiting the sample to cohorts graduating either before or after the compulsory schooling reform.

cohort fixed effects, δ_{cr}, in order to limit our comparisons to students born in the same year within a relatively local geographic region and capture general economic conditions that may affect each birth cohort's access to JEC and subsequent labor market outcomes.[17] As δ_{cr} varies by cohort, it accounts for national and regional-level changes in schooling requirements or education policy.[18]

To more formally explore the possibility of non-random timing at the municipality-level, we examine the extent to which JEC coverage is associated with contemporaneous economic conditions during the rollout period in Table 3.2. Specifically, for each year during the implementation period for which a CASEN survey was conducted (1996, 1998, 2000, 2003, and 2006), we regress the fraction of students in grades 1-12 attending a full-day school in municipality m with measures of city economic and demographic characteristics in that same year. Without including municipality fixed effects, Table 3.2 indicates full-day schooling was rolled out quickly in relatively low-populated areas with low levels of educational attainment (columns (1) and (2)). Columns (3) through (6) include municipality fixed effects in order to examine the extent to which increases in JEC coverage are associated with *changes* in a locality's economic conditions, and columns (5) and (6) explore the importance of alternative measures of disadvantage by including per-capita income, rather than poverty rates. Across specifications, increased JEC participation is associated with increased poverty rates, although this relationship somewhat sensitive to the measure of disadvantage (columns 3 and 4, versus 5 and 6) and economically small in magnitude: moving from 0 percent to 100 percent poverty is associated with an the share of students attending full-day schools increasing by 14 percentage points. Over the 1996 to 2006 period, the poverty rate in the median municipality fell by 12.5 percentage points, therefore, scaling the estimated coefficient by the (absolute) changes in poverty within a city over a decade implies a very small change in access to full-day schooling, relative to the full possible 12 years of exposure. Nonetheless, in order to account for the possibility that the timing of JEC adoption is correlated with changes in local poverty conditions, we include survey year trends in pre-reform (1996) municipality of birth poverty and employment rates. The appendix shows omitting these trends does not meaningfully affect our results.

We further explore the extent to which the introduction and pace of JEC is correlated with baseline characteristics regressing the first and last year of JEC implementation and the number of years it took for a municipality to go from 0-100% coverage using information from the 1992 Census in Appendix Table C.2, similar to the approach in Hoynes and Schanzenbach (2009), with the caveat that not all information is available for the smallest cities.[19] Appendix

[17]There are fifteen regions in our analysis period. Excluding the Santiago region, we identify city of birth for respondents in between four and 54 municipalities within a region. In total, each region outside of Santiago has between 100,000 and 1.8 million residents, compared to between about 200 and 300,000 per municipality.

[18]As JEC exposure does not vary among students from the same cohort living in the same municipality, we are unable to include cohort-specific fixed effects at more granular geographies.

[19]We use data from the last Census that was administered prior to the announcement of JEC rather than the CASEN as the Census provides information on more outcomes that are representative at the municipal-

Table C.2 shows that while larger areas had some JEC access relatively early (columns (3) and (4)), less populated areas moved to full implementation more quickly (columns (7) and (8)). Holding other factors constant, there is no significant correlation between the timing of adoption and other baseline characteristics. Even with regional fixed effects, however, more than 40 percent of variation in the pace of implementation is unexplained.

3.5 Findings

Educational Attainment

We first turn to explore whether access to additional school time changes educational attainment, as changes in educational attainment provide one mechanism for any patterns in earnings or labor force attachment in the long term. *A priori*, the effect of JEC on high school and college graduation is ambiguous. On one hand, less leisure time during high school reduces the ability of teenagers to hold part-time jobs, and increases the opportunity costs of attending school, which may increase drop out rates. On the other hand, if more time in elementary or secondary school prepares students for higher education or instills non-pecuniary benefits of schooling (a stronger "taste" for education), longer school days may increase educational attainment.

Tables 3.3 and 3.4 show how JEC changed educational attainment by estimating the cross-city, cross-cohort framework in Equation 3.3 on high school and university graduation, respectively. The effect of exposure to an additional year of full-day schooling is presented in the first row. There are several ways to scale these estimates to recover policy-relevant parameters. First, the effect for the average individual in our sample is obtained by multiplying this row by the average expected number of JEC years in each sample: about 2 years for college graduation, and 3 years for the slightly younger high school graduate sample. Alternatively, the implied effect of an additional year-equivalent of education, is recovered by scaling the main estimate by the average increase in instructional time under JEC ($\frac{\beta}{0.3}$).

For the full population, access to an additional year of full-day schooling increased the probability of high school graduation by 2.1 percentage points (column (1) of Table 3.3). The remaining columns explore whether these effects differ by gender or family socioeconomic status. Heterogeneity along these dimensions is of interest for several reasons. First, as maternal education is particularly important for child outcomes (Andrabi et al., 2013; Carneiro et al., 2011; Currie, 2009), any intergenerational benefits of full-day schooling are likely to arise through gains to women. Second, essentially all children from lower-income families attend government-subsidized, rather than private, schools, whereas most students from the highest-income families attend private schools (CASEN, 2016). Moreover, substantially fewer children from disadvantaged backgrounds move across municipalities between birth and

ity level than the CASEN. Even with this richer data, municipality-level information is only available for relatively populous areas. We view these results, although suggestive, as complementary to the findings in Table 3.2 using the CASEN data.

school start (18 vs. 28 percent). Therefore, estimated access to full-day schooling is more likely to reflect actual access among this population, and we may interpret lower-income students as a "high-complier" population where the reported ITT estimates are expected to be similar to the TOT effect. In addition, evidence from other educational interventions suggests the returns to educational inputs may be larger for lower-income students (Cunha et al., 2006; Havnes and Mogstad, 2011).

Greater access to full-day schooling increased high school graduation among all subgroups, particularly for women (Table 3.3 column (2)) and students from disadvantaged backgrounds ("low SES" in column (4)).[20] These qualitative patterns are robust to alternative samples and more parsimonious controls for local economic conditions (Appendix Table C.3.

Table 3.4 reveals different patterns for college graduation. Longer school days increase university graduation for all groups, but especially for men and higher SES populations. For men, an additional year of full-day schooling increases college completion by 1.9 percentage points (about 11 percent) for men and 1.8 percentage points (about 6 percent) for students from higher-SES families. In Appendix Table C.4 we examine whether JEC led students to receive at least some college education in order to explore whether the differences between high school graduation and college graduation are due to students not beginning college, or starting college but not yet completing. For women, the primary margin appears to be high school completion: there a small and weakly significant increase in the likelihood of receiving at least some college education. For men, these results are consistent with increases in college graduation documented in Table 3.4. Whereas additional time in elementary and secondary school prompted low-SES students to complete high school and enter college, higher SES students attend and complete college at higher rates.

During the JEC implementation period, the Chilean government enacted other changes to the educational environment. Beginning in 2003, free elementary and secondary education was guaranteed and compulsory for all individuals up to age 21. By requiring students to enroll in school through their teen years, this reform may have mechanically increased high school graduation rates. Importantly, region-by-cohort fixed effects will account for this national-level reform as expanded compulsory schooling affected all cohorts born in 1982 and later, regardless of the place of birth. Compared to older (1979-1981) birth cohorts, high school graduation rates for the post-1982 cohorts in our are 12 percentage points higher (83, versus 71, percent), suggesting the compulsory schooling reform was effective at increasing high school graduation rates. Even after the reform, however, not all individuals completed secondary school, suggesting imperfect compliance with compulsory schooling and the scope for other interventions to induce schooling completion. We obtain similar results for both secondary and tertiary education when we limit the sample to those subject to the secondary schooling law (cohorts born after 1981), suggesting that longer school days increased educa-

[20]The low SES sample is defined as individuals whose mothers have no more than a basic education, as reported by individuals and linked by family structure. We pool men and women from disadvantaged households; there are no substantial differences in outcomes by gender among this subpopulation.

tional attainment above and beyond the provisions in the secondary schooling reform (results available upon request).

A separate question is whether there are diminishing marginal benefits to additional years of full-day schooling or if there exists a threshold after which longer school days provide especially large benefits. To explore these patterns, Figure 3.3 plots the β_y coefficients from the less parametric approach in Equation 3.4 and shows access to *any* full-day schooling increases high school graduation rates by approximately 3 percentage points, with relatively small marginal increases for each subsequent year of full-day schooling. Figure 3.4 shows the likelihood of the full population is generally increasing in exposure to JEC, with each additional year of exposure conferring a smaller marginal gain. This aggregate pattern is clearer among men, and mirroring the results in Table 3.4, additional time in school does not increase college graduation rates among disadvantaged students, while those from more highly-educated families incur a one-time increase that further increases after about five years of exposure.

These figures also illustrate the distributional effects of JEC. Specifically, about 30 percent of our sample is exposed to less than 1 year of JEC, while about 5 percent have at least 6 years. The vertical distance from one year of JEC to the $[6, 7)$ point then roughly corresponds to changes going from the 30th percentile to 95th percentile of JEC access (about 10 log points for high school graduation and 6 log points for college graduation).

Labor market outcomes

The return to secondary schooling in Chile was large during the period JEC was introduced. The existing estimates of the high school wage premium during this period range from about 34 percent relative to those with an eighth grade education (8 percent per year of secondary education) to 64 percent (11 percent a year) relative to those with a sixth grade education (OECD, 2013; Manacorda et al., 2010). The estimated earnings premium for post-secondary education is even higher: Manacorda et al. (2010) find Chilean men with a university degree have higher labor force participation rates and earn 90 percent more than those with a secondary education. Since JEC increased educational attainment, we might expect improved economic outcomes when students in their 20s and 30s.

Employment

We first examine whether JEC changed employment rates, as the probability of working is increasing in educational attainment. In Table 3.5, we defined employment as whether an individual had at least 30,000 pesos in work income the previous month (approximately $50 in 2017 dollars).[21] For all subgroups, changes in employment are modest in magnitude, at about 1.3 percentage point or less from a base of 55 to 66 percent. While the estimate for disadvantaged populations and women suggest significant increases in employment on

[21] Results are qualitatively similar to defining work as employment in the week prior to the reference period.

the order of two percent, we do not find any significant change among students from high SES backgrounds.These results contrast with the findings of Pires and Urzua (2015) that does not observe any aggregate increase in employment. One difference for these findings is that Pires and Urzua (2015) focus on for students with access to full-day schooling only in the final years of high school and who are in their mid-20s at the time of the survey. As we find increases in educational attainment, our larger employment responses point to the importance of measuring labor market outcomes after respondents have reached an age where they are expected to have completed schooling.

Figure 3.5 takes a less parametric approach shows the probability of employment is generally increasing in access to full day schooling, with significant employment gains among women and disadvantaged students emerging with approximately two years of JEC access. In contrast, although estimates are insignificant throughout the JEC distribution, there is no evidence access to more full-day schooling changes employment among students from more educated backgrounds. In additional results we do not find a significant change in the probability young adults are currently in school, suggesting that the lack of an employment response among students from the highest-SES backgrounds is not driven by selection out of the labor force and into post-secondary schooling (results available upon request).

Earnings

Given increases in educational attainment and employment, we would expect that access to longer school days would increase earnings in early adulthood. To our knowledge, this study provides one of the first direct examinations of the relationship between earnings and full-day schooling for a full, large-scale national reform.[22]

Table 3.6 panel (a) reports the semi-elasticity of earnings with respect to an additional year of JEC, where earnings is measured as the log of earnings in the previous month, plus one in order to incldue individuals with no earnings.[23] Consistent with longer school days improving labor market outcomes, Table 3.6 shows additional time in school increases earnings, with each additional year of full-day access increasing earnings by 4-5 percent (columns (1) through (4)) for all groups except those from the most advantaged backgrounds. To put these numbers in context, as JEC increased instructional time by 30 percent, the results in column (1) suggest about a 16 percent ($\frac{0.048}{0.3}$) return to each year-equivalent of education. These magnitudes are on the higher end of the returns to education found in higher-income countries (Card, 1999) and consistent with the ranges found for Chile during the 1990s.

[22]Pires and Urzua (2015) examine labor market outcomes for students who were in high school when JEC was introduced and measure earnings when these students are in their mid-20s. Our study broadens our understanding of this relationship by examining labor market outcomes for students who had up to 12 years of access to longer schooling and tracking earnings through individuals' 20s and 30s.

[23]Appendix Table C.7 shows larger earnings gains using levels or earnings or an inverse hyperbolic sine transformation as the dependent variable.

In order to examine the full distribution of earnings responses – that is, whether any gains are concentrated among especially low- or high-wage ends of the labor market, Figure 3.6 displays results from a series of regressions where the outcome of interest is a binary variable whether an individual has annual earnings of at least x pesos, following Carrell et al. (2018). This approach incorporates both labor force participation and earnings responses like Table 3.6 panel (a). Figure 3.6 indicates that access to longer school days had particularly pronounced effects on the low end of the labor market, with negligible effects in increasing the likelihood an individual earned more than about 1.5 million pesos a month ($2,500, about the 97th percentile). The pattern is less monotonic for higher-SES individuals, for whom earnings gains are most pronounced between 0.6 and 1.2 million persons, consistent with these individuals having relatively high earnings regardless of JEC access.

Figure 3.7 plots the coefficients from the less parametric approach to investigate non-linearities access to longer school days and earnings. The figure shows for the overall, male, and disadvantaged populations, log earnings increase approximately linearly between about 2-8 years with little evidence of diminishing marginal returns. Regardless of how many years more advantaged students are likely to have access to JEC, there is no significant change in earnings.[24]

Finally, Table 3.6 panel (b) limits the sample to workers (defined as in Table 3.5) in order to examine whether the patterns in Figure 3.7 are driven by more individuals entering the workforce or higher earnings among the employed. Between half and 80 percent of the overall increase is due to higher earnings among the employed for women, men, and low-SES groups. For higher SES groups, we also find evidence of longer school days increasing earnings after conditioning on employment.

Mechanisms

There are several intermediate, non-mutually exclusive channels through which longer school days could increase earnings in adulthood. We previously documented one such mechanism – greater educational attainment. This section explores other potential channels.

Migration

One possible explanation for increased employment and earnings is that individuals with greater educational attainment have greater ability and financial resources to migrate from rural areas to Santiago and other areas where wages are higher. Table 3.7 investigates the relationship between JEC exposure and subsequently moving a municipality outside the municipality of birth at any point. For all populations, there is no significant change

[24]We have examined the effect of the reform on usual hours worked and found a marginally significant increase for this group of about 0.2 hours (less than three minutes a week). We interpret these results with some caution, as this variable is likely measured with error: half of workers in our sample report working exactly 45 hours in a typical week. An alternative explanation, which we are unable to explore with the available data, is changes in part-year or seasonal work.

in migration patterns from greater exposure to longer school days. Appendix Table C.9, column (1) shows that migration to Santiago, the largest metropolitan area, likewise did not change. Column (2) of Appendix Table C.9 takes a more general approach by multiplying an indicator for currently residing in a municipality other than the municipality of birth with a measure of economic prosperity in the current city, where we define economic prosperity as a standardized index based on the leave-out-mean individual income in each respondent's current municipality.[25] Here, we find individuals with greater access to full-day schooling, particularly those from more highly-educated families, tend to move to more prosperous areas.[26] In Section 3.5 we consider general equilibrium effects in order to analyze how JEC shaped the economic opportunities in an area.

Fertility patterns

For women, motherhood is associated with labor force non-participation and lower earnings upon labor market re-entry (Waldfogel, 1998; Kleven et al., 2018; Bertrand et al., 2010; Kuziemko et al., 2018). Previous work has documented that JEC leads to lower teen pregnancy rates for disadvantaged women in urban areas (Berthelon and Kruger, 2011). We also find small reductions in teen pregnancy, consistent with these earlier findings (column (1), Appendix Table C.10). When we estimate the effect of longer school days on the age at first birth among women who gave birth to at least one child, we find access to full-day schooling led women to give birth at older ages, consistent with reductions in teenage pregnancy. Each additional year of full-day schooling delayed birth by about two months (Table 3.8 and Figure 3.8). These results are slightly larger for lower-SES women (columns (2) vs. (3)). As the youngest individuals in our sample are in their early 20s and have not yet reached prime childbearing years, this estimate likely understates the full effect of JEC on family formation patterns.

Occupation choice

Another mechanism by which longer school days could increase earnings is through occupational choice. As the majority of additional school time under JEC went towards reading and math instruction, students attending full-day schools are expected to have entered the labor force with greater skills, even absent a formal credential. In table 3.9, we find longer school days increased the likelihood of having a managerial, professional, or technical occupation by about 1 percentage point (3.5 percent) for all individuals from non-disadvantaged backgrounds, while greater access to longer school days does not affect the share low-SES

[25]In particular, we calculate $\frac{\overline{y_{mt}}-\sum_m \sum_t \overline{y_{mt}}}{\sigma_{\overline{y}}}$ where $\overline{y_{mt}}$ is per capita income in municipality m surveyed at year t and $\sum_m \sum_t y_{mt}$ is the grand mean across all city-years, and $\sigma_{\overline{y}}$ is the corresponding standard deviation.

[26]As emigrants are not surveyed in CASEN, our findings also do not reflect international migration.

individuals in these roles.[27]

Since most "high-skilled" occupations in Table 3.9 require a university degree, Appendix Table C.11 column (1) explores an alternative measure of occupational prestige that captures upskilling across the entire skill distribution. For each individual, we measure the log earnings of other workers j in the same 4-digit occupation o as the leave-out mean:

$$\overline{w_{io}} = \frac{\sum_j w_{j \neq i}}{\sum_j N_{jo} - 1} \tag{3.5}$$

Increased access to full-day schooling increases the occupational prestige (measured by salary) for both men and women, as well as for higher-SES individuals. In contrast, there continues to be no relationship between the types of occupations held by lower-SES populations and exposure to full-day schools.

Family resources or academic skills?

In addition to increasing human capital accumulation for students, longer school days provide a source of child care for families. This implicit subsidy increases family resources by reducing the cost of child care and potentially allowing parents to enter the labor force or work longer hours rather than provide home-based care. Although the CASEN does not enquire whether a respondent's parents were employed during childhood, we provide suggestive evidence on the extent to which our findings are driven by increased parental employment by calculating the change in labor force participation rate among mothers with school-aged children in the 1996-2006 CASEN surveys at the municipal level. We then define a "high maternal LFP increase" sample comprised of municipalities that experienced greater-than-median increases in maternal labor supply over the first decade of JEC implementation (1996 to 2006). Cities in this subsample increased maternal labor force participation by at least 6.3 percentage points from a base of about 38 percent, and the average city in this sample increased maternal participation by about 12 percentage points.

Table 3.10 examines our main outcomes when we limit the sample to areas with large increases in maternal labor force participation. While we cannot rule out results of the magnitude found in the main findings for most outcomes, in general, Table 3.10 does not show benefits were exclusively found in areas with particularly large increases in maternal labor supply. While we are unable to directly account for changes in family income and labor supply at an individual level, these results suggest that our main findings are not exclusively driven through changes in family resources during childhood.

General equilibrium considerations and robustness

During the 1990s and early 2000s, Chile underwent a period of political stability, deregulation, and economic growth. Policymakers across the political spectrum advocated policies

[27] Following ILO, we define "skilled" occupations as the primary occupation in managerial, professional, and technical occupations (major codes 1, 2, and 3).

to alleviate poverty and open the country's economy to trade (Foxley, 2004). Similar economic reforms occurred in much of Latin America and Eastern Europe over this period, and continue in many emerging economies today. Therefore, our results arguably generalize to other settings.

A separate question is the extent to which our findings have internal validity, and in particular, whether our measure of JEC access is capturing other local economic changes that affect labor market outcomes. Columns 5 and 6 of Table 3.2 indicate that after conditioning on year and city fixed effects, city-level labor market and demographic characteristics at the time of JEC implementation are not significantly associated with the pace of JEC adoption; here we further explore this issue by examining the relationship between full-day schooling and the entire local economy in the long-run.

JEC was a large-scale reform increasing classroom time up to 30 percent and eventually covering all students attending publicly-funded schools. Given the nature of the program, the partial equilibrium effects on the treated cohorts – the internal rate of return – may understate the full return to an additional year of schooling. Specifically, Table 3.3 showed JEC increased educational attainment, thereby increasing the size of the skilled labor force. In standard economic models, this increase in skilled labor supply is expected to reduce the earnings of skilled workers relative to those with less education (Goldin and Katz, 2009). To the extent that younger and older workers are imperfect substitutes, examining spillover effects to skilled and unskilled older workers can provide a sense of the magnitude of any general equilibrium effects (Khanna, 2015).[28]

In order to estimate the presence of general equilibrium and spillover effects of additional schooling, we augment Equation 3.3 by adding the average years of JEC exposure among the full adult (ages 18 and older) population and labor force:

$$y_{icmt} = \beta_1(\widehat{JEC_{cm_b}}) + \beta_2\overline{(\widehat{JEC_{m_lt}})} + X'_{icm_lm_bt}\gamma + Z'_{m_bt}\theta + \delta_{cr_b} + \phi_t + \psi_{m_b} + \psi_{m_l} + \varepsilon_{icm_lm_bt} \quad (3.6)$$

where now m_l denotes the municipality in which individual i currently lives and m_b denotes his or her municipality of birth. As before, β_1 captures the private returns to an additional year of full-day schooling. $\overline{(\widehat{JEC_{m_lt}})}$ is the average exposure among adults living in municipality m_l at survey period t, and β_2 captures general spillover effects of the aggregate increase in educational attainment. As spillover effects are based on individuals' current city of residence, this framework incorporates all migration decisions. We include region of birth-by-cohort, municipality of birth, municipality of residence, and survey year fixed effects, as well as the standard set of individual and city controls, X' and Z', from Equation 3.3.[29]

We estimate Equation 3.6 separately by skill level (those with less than a high school diploma, two measures of "high-skilled: those who graduated high school and those who

[28]We thank the editor for this suggestion.

[29]Applying the model outlined in Khanna (2015) is not feasible in this setting, as all of our "young" cohorts receive some exposure to longer school days – that is, there are no purely "untreated" municipalities.

completed college) and by age (individuals who were school-aged when the reform was introduced, "young" birth cohorts 1979-1993, and those who had already entered the labor market, "old" birth cohorts 1954-1978). As we have labor market information from multiple CASEN waves and m_l is not perfectly collinear with m_b, β_1 and β_2 are separately identified for individuals attending school during the implementation period. For the old cohorts who graduated high school before JEC was announced, we can only identify the parameter associated with the spillover effects,β_2.

Table 3.11 shows the extent to exposing an entire population (panel (a)) or workforce (panel (b)) to longer school days affects each skill category and generation. For young cohorts in columns (1) through (3), we continue to see the internal return to education is positive and significant, on the order of about 2 percent a year (column (2)), or slightly more than half of the earnings gains estimated in Table 3.6. In contrast, the internal returns are small in magnitude and insignificant for both the lowest- and highest-skilled groups. Spillover effects from the overall population point to positive externalities for high-skilled workers, while any spillover effect on the least-skilled young workers is sensitive to whether aggregate access to full-day schools is measured across the population or the workforce. For older individuals, we find no significant or consistent effects across different educational groups.

Overall, these results are consistent with higher levels of education in the labor force facilitating sectoral shifts and facilitating agglomeration economies that stimulate the demand for relatively skilled workers. These patterns across age groups also suggest old and young workers are imperfect substitutes and that any negative externalities from a larger young, relatively-skilled workforce are small in this context at least for the first 20 years of the reform. As students exposed to additional years of JEC enter the labor force and progress in their careers, these dynamics may change.

As a related exercise, we conduct a placebo analysis on cohorts born between 1959 and 1973 who completed secondary schooling before 1997 and therefore did not have access to full-day schooling. Our placebo measure of JEC access is arbitrarily set at the expected number of JEC years received by the cohort born twenty years later in the same municipality:

$$(JE\widehat{C_{cm,placebo}}) = \frac{1}{N_m} \sum_{s \in g} \sum_{g=1}^{12} \mathbb{1}\left\{JEC_{sgm,(c+20)}\right\} * N_{sgm} \tag{3.7}$$

If our main results were simply capturing changes in local economic growth, we would expect to see improvements in labor market outcomes for these older individuals. Appendix Table C.12 does not show any economically or statistically significant changes in college graduation, earnings, skilled occupation or age at first birth for any subgroup. Further, across all outcomes, the point estimates for this placebo sample are smaller in magnitude than those for students exposed to the reform. Although suggestive, combined with the general equilibrium analysis, this exercise indicates that our findings are not capturing changes in local economic conditions affecting the entire workforce.

3.6 Conclusion

We find that access to longer school days improves long-term economic well-being. Examining a large-scale national reform, we document that full-day schooling increases educational attainment, prompts more women and students from disadvantaged backgrounds to enter the labor force, and generates earnings gains on the order of 4-5 percent a year. The magnitude of these earnings gains is consistent with other work examining the returns to education in Chile during this time period. The margins of adjustment vary by subgroup: students from lower SES families are more likely to enroll in college and enter the workforce, whereas those from more advantaged backgrounds complete college, work in high-skilled occupations, and live in wealthy areas at higher rates.

These results are consistent with longer school days promoting greater human capital development, as suggested by school reports that most of the additional time was dedicated to instructional activity. In our data, we do not observe systematic changes in migration patterns, and general equilibrium effects are imprecisely estimated and relatively modest in magnitude for most groups. Finally, we do not observe especially large improvements in areas that experienced the largest increases in maternal employment during the JEC rollout period, suggesting that our findings are not solely due to increases in family resources or parental employment.

While access to additional time in school benefits students, a broader question is whether such large-scale investments are worthwhile from a social welfare viewpoint. Extending the school day on a national level for all students requires substantial resources. In our setting, the move to full-day schooling increased per-pupil expenditures by at least 20 percent (an increase of approximately 18,000 pesos (31 USD) per student each month). Extrapolating our estimated earnings increase in Table 3.6 panel (a), we estimate additional earnings for students attending school in the first twenty years of the reform are between 60 and 120 percent as large as the increase in per-pupil spending over this period. In the steady-state (e.g.: after full implementation), we estimate the cost to government in providing twelve years of longer school days is about 10 percent the discounted value increase in earnings over a student's full career (ages 23 to 65).[30] This back-of-the-envelope calculation is not a full cost-benefit analysis – it does not include costs of infrastructure, maintenance or teacher hiring, nor does it include benefits accruing from delayed childbearing or reduced crime (Berthelon and Kruger, 2011), but it does illustrate that many important benefits of educational investments are only realized in the long-run, while costs are primarily incurred in the short-term. Altogether, the broad-based nature of our results shows that large-scale investments in public education can generate long-term and meaningful improvements in economic well-being.

[30]We obtain a similar range when calculating the net costs of the first twenty years of implementation. Each of these estimates assume a 3 percent social discount rate.

3.7 Tables and Figures

Table 3.1: Summary Statistics: Main Adult Sample

	(1) All	(2) Women	(3) Men	(4) Low SES	(5) High SES
		Demographic characteristics			
$\widehatJEC$	2.020	2.008	2.034	1.686	2.086
	(2.143)	(2.148)	(2.136)	(1.960)	(2.061)
Age	28.20	28.27	28.12	27.64	27.76
	(3.897)	(3.924)	(3.867)	(3.486)	(3.717)
Year of birth	1984.8	1984.7	1984.8	1984.0	1985.2
	(3.916)	(3.911)	(3.921)	(3.541)	(3.924)
Female	0.517	1	0	0.524	0.520
	(0.500)	(0)	(0)	(0.499)	(0.500)
Indigenous	0.104	0.107	0.101	0.133	0.0670
	(0.306)	(0.309)	(0.302)	(0.339)	(0.250)
Married	0.187	0.214	0.159	0.185	0.174
	(0.390)	(0.410)	(0.365)	(0.389)	(0.379)
Civil partnership or married	0.444	0.471	0.414	0.421	0.390
	(0.497)	(0.499)	(0.493)	(0.494)	(0.488)
Has own children (parent)	0.582	0.692	0.463	0.618	0.514
	(0.493)	(0.461)	(0.499)	(0.486)	(0.500)
Number of children	0.918	1.114	0.708	0.986	0.766
	(0.978)	(0.994)	(0.916)	(0.996)	(0.906)
Mother has $¡$ HS education	0.440	0.447	0.432	1	0
	(0.496)	(0.497)	(0.495)	(0)	(0)
College-educated mother	0.0739	0.0679	0.0804	0	0.212
	(0.262)	(0.252)	(0.272)	(0)	(0.409)

Table 3.1: (continued)

	(1) All	(2) Women	(3) Men	(4) Low SES	(5) High SES
Economic well-being					
HS graduate	0.792	0.804	0.779	0.689	0.930
	(0.406)	(0.397)	(0.415)	(0.463)	(0.256)
College graduate	0.184	0.202	0.164	0.0980	0.308
	(0.387)	(0.401)	(0.371)	(0.297)	(0.461)
Worked last month	0.655	0.542	0.776	0.632	0.651
	(0.475)	(0.498)	(0.417)	(0.482)	(0.477)
Usual weekly hours worked last month	27.86	21.44	34.74	27.11	27.07
	(23.34)	(22.25)	(22.51)	(23.60)	(23.27)
Monthly earnings (1000s of 2017 pesos)	318.595	233.708	409.524	232.793	414.111
	(504.146)	(377.343)	(598.257)	(321.578)	(686.363)
Skilled occupation	0.292	0.379	0.226	0.172	0.456
	(0.455)	(0.485)	(0.418)	(0.378)	(0.498)
Residence characteristics					
Lives in urban area	0.852	0.854	0.849	0.779	0.942
	(0.355)	(0.353)	(0.358)	(0.415)	(0.234)
Lives in Santiago	0.110	0.112	0.108	0.0751	0.142
	(0.313)	(0.316)	(0.310)	(0.263)	(0.349)
Lives in different city	0.357	0.365	0.349	0.298	0.410
than city of birth	(0.479)	(0.481)	(0.477)	(0.457)	(0.492)
Employt rate	0.583	0.583	0.583	0.567	0.584
(city of residence)	(0.071)	(0.071)	(0.071)	(0.070)	(0.070)
Poverty rate	0.135	0.135	0.134	0.160	0.121
(city of residence)	(0.082)	(0.083)	(0.082)	(0.087)	(0.073)
Observations	157698	81210	76488	62767	48642

Notes: Table shows summary statistics for our full sample (column 1); women (column 2); men (column 3); individuals from disadvantaged backgrounds, defined as those whose mothers have less than a high school education (column 4); and individuals from advantaged backgrounds, defined as those whose mothers have at least a high school education (column 5). Expected JEC calculated from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2006-2017 CASEN surveys, using regionally-representative weights. Sample limited to individuals born between 1979 and 1992 outside the Santiago metropolitan region who were 23-38 years old at the time of survey. See text for details.

Table 3.2: Predictive Characteristics of JEC Adoption: Economic Characteristics during the Roll-out Period

	(1) pct JEC	(2) pct JEC	(3) pct JEC	(4) pct JEC	(5) pct JEC	(6) pct JEC
Employment rate, adults 18 +	0.2371	0.0688	0.0745	0.0245	0.0043	-0.0425
	(0.1944)	(0.2120)	(0.1551)	(0.1657)	(0.1574)	(0.1679)
Avg yrs education, adults 18 +	-0.0262***	-0.0377***	-0.0066	-0.0111	-0.0105	-0.0144
	(0.0065)	(0.0076)	(0.0108)	(0.0117)	(0.0118)	(0.0128)
Avg hh size	0.0184	0.0346	0.0238	0.0306	0.0349	0.0415
	(0.0271)	(0.0296)	(0.0301)	(0.0337)	(0.0293)	(0.0328)
Poverty rate	-0.0097	-0.0610	0.1366*	0.1403*		
	(0.0764)	(0.0849)	(0.0774)	(0.0846)		
Log population	-0.0220*	-0.0059	0.0056	0.0108	0.0040	0.0087
	(0.0117)	(0.0136)	(0.0139)	(0.0169)	(0.0140)	(0.0170)
Log autonomous income					0.0019	0.0010
					(0.0241)	(0.0261)
Observations	1368	1121	1368	1121	1368	1121
R-squared	0.692	0.676	0.880	0.874	0.879	0.874
Year FE	X	X	X	X	X	X
Municipality FE			X	X	X	X
Includes Santiago	X		X		X	

Notes: Dependent variable is the fraction of students in grades 1-12 attending a full-day school at the municipality level for each year during the JEC rollout period the CASEN was administered (1996, 1998, 2000, 2003, and 2006). Employment rate defined as the share of adults ages 18 and older in a municipality with at least 30,000 peso earnings in the last month (approximately $50); average years of education is the average number of years adults ages 18 and older in a municipality attended school from grades kindergarten through 16 (four years of university); poverty rate defined as the share of individuals in a municipality with household income below a minimum subsistence level, based on a food expenditures; autonomous income defined as per capita income in a municipality from all household sources, primarily earnings, and also rental income. Odd-numbered columns present results for all municipalities and even-numbered columns exclude municipalities in the Santiago metropolitan region. Robust standard errors clustered by municipality. *** $= p < 0.01$, ** $= p < 0.05$, * $= p < 0.10$

Table 3.3: Longer School Days and High School Graduation

	(1) All	(2) Women	(3) Men	(4) Low SES	(5) High SES
$\widehat{JEC}$	0.021***	0.024***	0.017***	0.022***	0.008***
	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)
Observations	248535	126929	121606	113172	79169
DV mean	0.794	0.808	0.779	0.701	0.926
Pct change	0.026	0.030	0.022	0.032	0.009
E($\widehat{JEC}$)	2.925	2.902	2.949	2.757	3.083

Notes: Dependent variable is an indicator equal to one if the respondent had completed high school at the time of the CASEN survey. All specifications include city of birth, survey year, and birth year-by-region fixed effects. Control variables include current municipality of residence employment and poverty rates, gender, a quadratic in age, indigenous identity, and maternal education, as well as survey year linear trends in baseline poverty and employment rates by municipality of birth from the 1996 CASEN. $\widehat{JEC}$ defined as the expected years of full-day school attendance based on an individual's city and year of birth, calculated as described in Equation 3.1 from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2006-2017 CASEN surveys. Sample limited to individuals born between 1979 and 1992 outside the Santiago metropolitan region who were 19-38 years old at the time of survey. Columns (2) and (3) limit the sample to women and men, and columns (4) and (5) limit the sample to individuals whose mothers had less than a high school education or at least a high school education, respectively. Individuals not reporting maternal educational attainment are excluded from columns (4) and (5). Robust standard errors clustered by city of birth; all specifications weighted using regionally-representative weights. See text for details. *** $= p < 0.01$, ** $= p < 0.05$, * $= p < 0.10$.

Table 3.4: Longer School Days and College Graduation

	(1) All	(2) Women	(3) Men	(4) Low SES	(5) High SES
$\widehat{JEC}$	0.014***	0.009***	0.019***	0.004**	0.018***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.005)
Observations	172681	88972	83709	77796	52510
DV mean	0.182	0.199	0.164	0.0992	0.309
Pct change	0.075	0.044	0.114	0.044	0.059
E($\widehat{JEC}$)	1.958	1.944	1.973	1.768	2.041

Notes: Dependent variable is an indicator equal to one if the respondent had received a university degree at the time of the CASEN survey. All specifications include city of birth, survey year, and birth year-by-region fixed effects. Control variables include current municipality of residence employment and poverty rates, gender, a quadratic in age, indigenous identity, and maternal education, as well as survey year linear trends in baseline poverty and employment rates by municipality of birth from the 1996 CASEN. $\widehat{JEC}$ defined as the expected years of full-day school attendance based on an individual's city and year of birth, calculated as described in Equation 3.1 from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2006-2017 CASEN surveys. Sample limited to individuals born between 1979 and 1992 outside the Santiago metropolitan region who were 22-38 years old at the time of survey. Columns (2) and (3) limit the sample to women and men, and columns (4) and (5) limit the sample to individuals whose mothers had less than a high school education or at least a high school education, respectively. Individuals not reporting maternal educational attainment are excluded from columns (4) and (5). Robust standard errors clustered by city of birth; all specifications weighted using regionally-representative weights. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table 3.5: Longer School Days and Employment in the Previous Month

	(1) All	(2) Women	(3) Men	(4) Low SES	(5) High SES
$\widehat{JEC}$	0.009***	0.013***	0.006*	0.011***	-0.008
	(0.003)	(0.004)	(0.003)	(0.003)	(0.005)
Observations	157696	81210	76486	70419	48641
DV mean	0.655	0.542	0.776	0.649	0.651
Pct change	0.014	0.025	0.008	0.017	-0.012
E($\widehat{JEC}$)	2.021	2.008	2.034	1.872	2.086

Notes: Employment defined as having income at least 30,000 pesos (approximately $50) in the past month. All specifications include city of birth, survey year, and birth year-by-region fixed effects. Control variables include current municipality of residence employment and poverty rates, gender, a quadratic in age, indigenous identity, household size, maternal education, marital status and number and presence of children, interacted with gender, as well as linear survey year trends in baseline poverty and employment rates by municipality of birth from the 1996 CASEN. $\widehat{JEC}$ defined as the expected years of full-day school attendance based on an individual's city and year of birth, calculated as described in Equation 3.1 from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2006-2017 CASEN surveys. Sample limited to individuals born between 1979 and 1992 outside the Santiago metropolitan region who were 23-38 years old at the time of survey. Columns (2) and (3) limit the sample to women and men, and columns (4) and (5) limit the sample to individuals whose mothers had less than a high school education or at least a high school education, respectively. Individuals not reporting maternal educational attainment are excluded from columns (4) and (5). Robust standard errors clustered by city of birth; all specifications weighted using regionally-representative weights. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table 3.6: Longer School Days and Log Monthly Earnings

	(1) All	(2) Women	(3) Men	(4) Low SES	(5) High SES
Panel a: All					
$\widehat{JEC}$	0.048***	0.049***	0.048***	0.039***	0.009
	(0.009)	(0.012)	(0.010)	(0.009)	(0.018)
Observations	157696	81210	76486	70419	48641
DV mean (level, 1000s pesos)	318.596	233.708	409.536	246.378	414.115
E($\widehat{JEC}$)	2.021	2.008	2.034	1.872	2.086
Panel b: Workers					
$\widehat{JEC}$	0.036***	0.032***	0.038***	0.019***	0.043***
	(0.006)	(0.008)	(0.006)	(0.006)	(0.013)
Observations	101839	42245	59594	44852	31351
DV mean (level, 1000s pesos)	486.183	430.530	527.866	379.556	635.665
E($\widehat{JEC}$)	1.904	1.910	1.900	1.824	1.883

Notes: Log monthly earnings are defined as $log(earnings + 1)$ (in 2017 pesos) in order to account for individuals with no earnings. All specifications include city of birth, survey year, and birth year-by-region fixed effects. Control variables include current municipality of residence employment and poverty rates, gender, a quadratic in age, indigenous identity, household size, maternal education, marital status and number and presence of children, interacted with gender, as well as linear survey year trends in baseline poverty and employment rates by municipality of birth from the 1996 CASEN. $\widehat{JEC}$ defined as the expected years of full-day school attendance based on an individual's city and year of birth, calculated as described in Equation 3.1 from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2006-2017 CASEN surveys. Sample limited to individuals born between 1979 and 1992 outside the Santiago metropolitan region who were 23-38 years old at the time of survey. Panel (b) is limited to individuals with earned income of at least 30,000 pesos in the previous month. Columns (2) and (3) limit the sample to women and men, and columns (4) and (5) limit the sample to individuals whose mothers had less than a high school education or at least a high school education, respectively. Individuals not reporting maternal educational attainment are excluded from columns (4) and (5). Robust standard errors clustered by city of birth; all specifications weighted using regionally-representative weights. See text for details. *** $= p < 0.01$, ** $= p < 0.05$, * $= p < 0.10$.

Table 3.7: Longer School Days and Cross-Municipality Migration

	(1) All	(2) Women	(3) Men	(4) Low SES	(5) High SES
$\widehat{JEC}$	-0.001	-0.001	-0.002	0.000	0.000
	(0.003)	(0.004)	(0.003)	(0.004)	(0.005)
Observations	157696	81210	76486	70419	48641
DV mean	0.357	0.365	0.349	0.295	0.410
Pct change	-0.004	-0.002	-0.005	0.001	0.000
E($\widehat{JEC}$)	2.021	2.008	2.034	1.872	2.086

Notes: Dependent variable is an indicator equal to one if an individual resided in a municipality other than his or her city of birth at the time of the CASEN survey. All specifications include city of birth, survey year, and birth year-by-region fixed effects. Control variables include current municipality of residence employment and poverty rates, gender, a quadratic in age, indigenous identity, household size, maternal education, marital status and number and presence of children, interacted with gender, as well as linear survey year trends in baseline poverty and employment rates by municipality of birth from the 1996 CASEN. $\widehat{JEC}$ defined as the expected years of full-day school attendance based on an individual's city and year of birth, calculated as described in Equation 3.1 from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2006-2017 CASEN surveys. Sample limited to individuals born between 1979 and 1992 outside the Santiago metropolitan region who were 23-38 years old at the time of survey. Columns (2) and (3) limit the sample to women and men, and columns (4) and (5) limit the sample to individuals whose mothers had less than a high school education or at least a high school education, respectively. Individuals not reporting maternal educational attainment are excluded from columns (4) and (5). Robust standard errors clustered by city of birth; all specifications weighted using regionally-representative weights. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table 3.8: Longer School Days and Age at First Birth (Women)

	(1) All women	(2) Low SES	(3) High SES	(4) Urban
$\widehat{JEC}$	0.156***	0.095**	0.073	0.159***
	(0.032)	(0.045)	(0.066)	(0.038)
Observations	54128	21523	16081	42545
DV mean	21.13	20.70	21.54	21.22
E($\widehat{JEC}$)	2.462	2.185	2.302	2.370

Notes: Dependent variable is the age in years a woman gave birth to her first child. All specifications include city of birth, survey year, and birth year-by-region fixed effects. Control variables include current municipality of residence employment and poverty rates, gender, a quadratic in age, indigenous identity, and maternal education, as well as survey year linear trends in baseline poverty and employment rates by municipality of birth from the 1996 CASEN. $\widehat{JEC}$ defined as the expected years of full-day school attendance based on an individual's city and year of birth, calculated as described in Equation 3.1 from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2006-2017 CASEN surveys. Sample limited to women born between 1979 and 1992 outside the Santiago metropolitan region who had given birth to at least one child at the time of the survey. Column (2) limits the sample to women whose mothers had less than a high school education; column (3) limits the sample to women whose mothers had at least a high school education; column (4) limits the sample to women born in urban areas. Robust standard errors clustered by city of birth; all specifications weighted using regionally-representative weights. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table 3.9: Longer School Days and Working in a Skilled Occupation

	(1) All	(2) Women	(3) Men	(4) Low SES	(5) High SES
$\widehat{JEC}$	0.010***	0.010*	0.009***	-0.001	0.016**
	(0.003)	(0.005)	(0.003)	(0.004)	(0.007)
Observations	101209	42220	58989	44828	30829
DV mean	0.292	0.379	0.226	0.179	0.456
Pct change	0.034	0.025	0.042	-0.006	0.035
E($\widehat{JEC}$)	1.894	1.899	1.890	1.812	1.874

Notes: Dependent variable is an indicator equal to one if the respondent is employed in a skilled occupation, defined as a managerial, technical, or professional occupation, following ILO. Military members and respondents without valid occupation codes are excluded from the analysis. All specifications include city of birth, survey year, and birth year-by-region fixed effects. Control variables include current municipality of residence employment and poverty rates, gender, a quadratic in age, indigenous identity, household size, maternal education, marital status and number and presence of children, interacted with gender, as well as linear survey year trends in baseline poverty and employment rates by municipality of birth from the 1996 CASEN. $\widehat{JEC}$ defined as the expected years of full-day school attendance based on an individual's city and year of birth, calculated as described in Equation 3.1 from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2006-2017 CASEN surveys. Sample limited to individuals born between 1979 and 1992 outside the Santiago metropolitan region who were 23-38 years old at the time of survey. Columns (2) and (3) limit the sample to women and men, and columns (4) and (5) limit the sample to individuals whose mothers had less than a high school education or at least a high school education, respectively. Individuals not reporting maternal educational attainment are excluded from columns (4) and (5). Robust standard errors clustered by city of birth; all specifications weighted using regionally-representative weights. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table 3.10: Long-term Economic Well-being in Municipalities with Largest Increases in Maternal LFP

	(1)	(2)	(3)	(4)	(5)	(6)
	HS grad	College	Work last year	Log(earn)	Skilled occupation	Age at 1st birth
			Panel a: All			
$\widehat{JEC}$	0.015***	0.011*	0.006	0.037*	0.007	0.021
	(0.005)	(0.006)	(0.007)	(0.022)	(0.005)	(0.052)
Observations	76352	52067	50694	50694	32574	14132
DV mean	0.808	0.188	0.646	333396.4	0.308	21.49
Pct change	0.018	0.058	0.009		0.022	0.001
E($\widehat{JEC}$)	2.893	1.972	1.919	1.919	1.789	1.880
			Panel b: Women			
$\widehat{JEC}$	0.021***	0.007	0.015	0.051*	0.006	0.021
	(0.006)	(0.006)	(0.009)	(0.029)	(0.010)	(0.052)
Observations	38974	26735	26069	26069	13656	14132
DV mean	0.818	0.206	0.533	244783.8	0.401	21.49
Pct change	0.025	0.032	0.027		0.015	0.001
E($\widehat{JEC}$)	2.880	1.967	1.915	1.915	1.805	1.880
			Panel c: Men			
$\widehat{JEC}$	0.009	0.015**	-0.002	0.031	0.007	
	(0.006)	(0.006)	(0.007)	(0.020)	(0.006)	
Observations	37378	25332	24625	24625	18918	
DV mean	0.797	0.168	0.768	428450.2	0.238	
Pct change	0.012	0.089	-0.003		0.030	
E($\widehat{JEC}$)	2.906	1.977	1.922	1.922	1.777	

Table 3.10: (continued)

	(1)	(2)	(3)	(4)	(5)	(6)
	HS grad	College	Work last year	Log(earn)	Skilled occupation	Age at 1st birth
Panel d: Low SES						
$\widehat{JEC}$	0.020***	-0.000	0.016**	0.046***	-0.005	0.092
	(0.006)	(0.005)	(0.007)	(0.017)	(0.008)	(0.095)
Observations	31216	21074	20489	20489	13075	5120
DV mean	0.710	0.0974	0.635	247697.9	0.183	20.90
Pct change	0.028	-0.003	0.024		-0.027	0.004
E($\widehat{JEC}$)	2.702	1.779	1.760	1.760	1.698	1.695
Panel e: High SES						
$\widehat{JEC}$	-0.001	0.010	-0.019**	-0.022	0.007	-0.203**
	(0.005)	(0.009)	(0.008)	(0.027)	(0.012)	(0.083)
Observations	27523	17986	17557	17557	11148	4717
DV mean	0.924	0.299	0.648	430709.1	0.447	21.90
Pct change	0.00	0.03	-0.03		0.02	-0.01
E($\widehat{JEC}$)	3.023	2.026	1.970	1.970	1.763	1.753

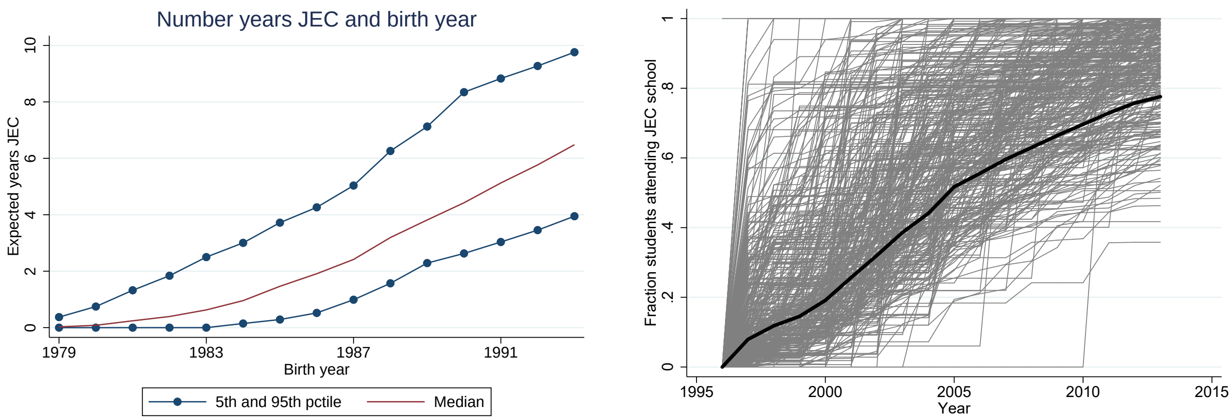
Notes: Dependent variables are as defined in Tables 3.3 through 3.9. Sample limited to respondents who were born in a municipality experiencing at least a 6.3 percentage point increase in the maternal labor force participation rate over the first decade of JEC implementation (1996-2006, from a base rate of approximately 38 percent). All specifications include city of birth, survey year, and birth year-by-region fixed effects. Control variables include current municipality of residence employment and poverty rates, gender, a quadratic in age, indigenous status, maternal education, as well as survey year linear trends in baseline poverty and employment rates by region of birth from the 1996 CASEN that vary by survey year. Columns (3)-(6) additionally include controls for household size, marital status and number and presence of children, interacted with gender. $\widehat{JEC}$ defined as the expected years of full-day school attendance based on an individual's city and year of birth, calculated as described in Equation 3.1 from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2006-2017 CASEN surveys. Sample limited to individuals born between 1979 and 1992 outside the Santiago metropolitan region who were ages 19-38 (column (1)); 22-38 (column (2)); 23-38 (columns (3)-(5)); or who had given birth (column (6)) at the time of survey and Panels (b) and (c) limit the sample to women and men, and panels (d) and (e) limit the sample to individuals whose mothers had less than a high school education or at least a high school education, respectively. Individuals not reporting maternal educational attainment are excluded from panels (d) and (e). Robust standard errors clustered by city of birth; all specifications weighted using regionally-representative weights. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table 3.11: General Equilibrium Effects of Longer School Days on Log Earnings

	(1)	(2)	(3)	(4)	(5)	(6)
	Young cohorts			Old cohorts		
	< HS	≥ HS	≥ BA	< HS	≥ HS	≥ BA
Panel a: $\overline{JEC}$ in population						
$\overline{\widehat{JEC}}$	-0.017	0.052	-0.111	0.018	0.026	-0.035
	(0.043)	(0.052)	(0.097)	(0.027)	(0.046)	(0.106)
$\widehat{JEC}$	0.001	0.025**	0.005			
	(0.014)	(0.011)	(0.024)			
Observations	37959	118261	24745	215239	173966	38057
DV mean (level, 1000s pesos)	174.915	355.971	693.866	201.675	529.163	1027.201
E($\overline{\widehat{JEC}}$)	1.834	1.801	1.791	1.820	1.777	1.753
Panel b: $\overline{JEC}$ in labor force						
$\overline{\widehat{JEC}}$	0.108**	0.184***	0.054	-0.015	-0.053	0.123
	(0.042)	(0.040)	(0.097)	(0.022)	(0.036)	(0.076)
$\widehat{JEC}$	-0.001	0.020*	0.005			
	(0.014)	(0.012)	(0.025)			
Observations	37958	118261	24745	215177	173954	38056
DV mean (level, 1000s pesos)	174.919	355.971	693.866	201.680	529.167	1027.211
E($\overline{\widehat{JEC}}$)	1.074	1.117	1.127	0.968	0.947	0.861

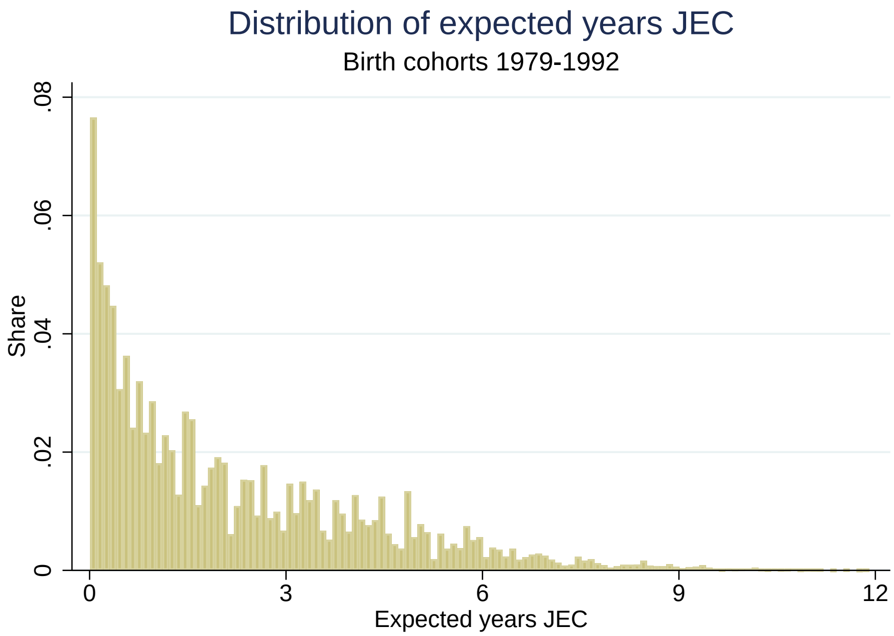
Notes: Dependent variable is the natural log of earnings in the previous month plus one to account for respondents not in the workforce. All specifications include city of birth, city of residence, survey year, and birth year-by-region fixed effects. Control variables include gender, a quadratic in age, indigenous identity, household size, maternal education, marital status and number and presence of children, interacted with gender, as well as linear survey year trends in baseline poverty and employment rates by municipality of birth from the 1996 CASEN. $\widehat{JEC}$ defined as the expected years of full-day school attendance based on an individual's city and year of birth, calculated as described in Equation 3.1 from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2006-2017 CASEN surveys. $\overline{\widehat{JEC}}$ defined as the average expected years of full-day school attendance based on an individual's city and year of birth, calculated as described in Equation (1) from enrollment and JEC adoption data from the Ministry of Education for the population ages 18 and older (panel (a)) and the labor force ages 18 and older (panel (b)). "Young" sample (columns (1-3)) limited to individuals born between 1979 and 1992 outside the Santiago metropolitan region who were ages 23-38 at the time of survey. "Old" sample limited to individuals born between 1955 and 1978 outside the Santiago metropolitan region who were ages 28-60 at the time of the survey. Columns (1) and (4) examine individuals with less than a high school degree; columns (2) and (5) consider those with at least a high school degree; columns (3) and (6) are limited to individuals with at least a four-year university degree. Two-way robust standard errors clustered by city of birth and city of residence. All specifications weighted using regionally-representative weights. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Figure 3.1: JEC Timing Varied across Municipalities



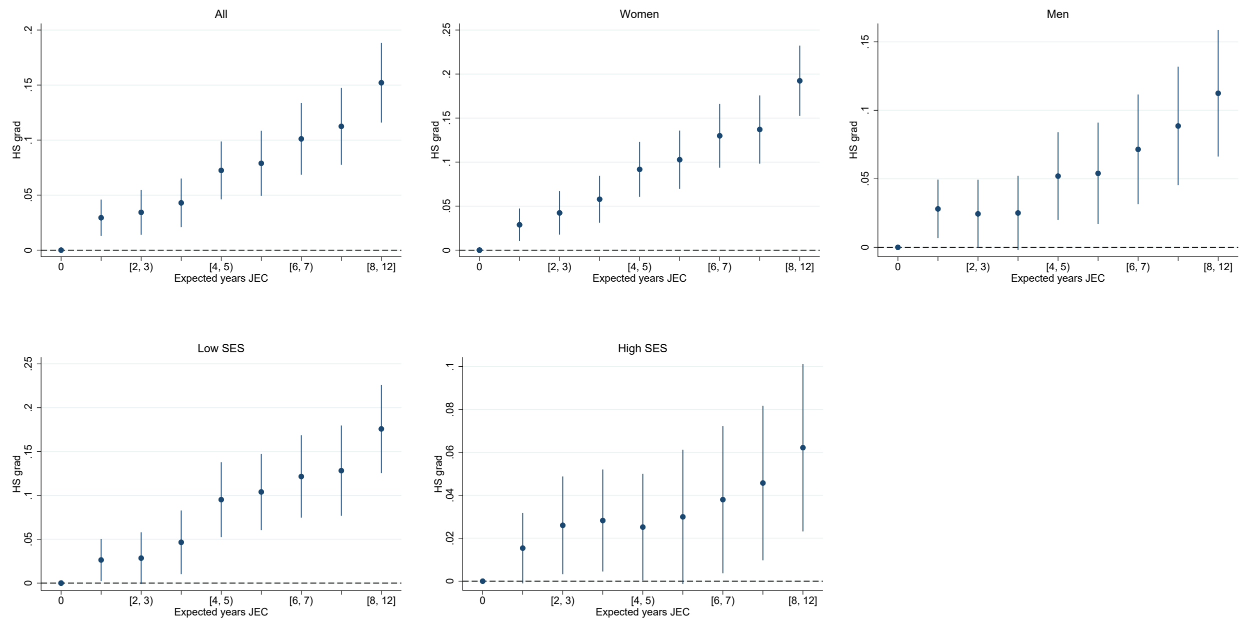
Notes: Figures shows the expected years of attending a full-day school across cities of birth by birth year. Panel (a): The bottom solid blue line denotes the expected years of full-day schooling in grades 1-12 for students at the 5th percentile of their cohort-specific distribution. The top blue line denotes the expected years of full-day schooling for students in the 95th percentile, and the red line shows the median expected years of exposure to the reform. Panel (b): Figure shows the fraction of students in grades 1-12 attending a JEC school in a given year with each line showing the pace of implementation for a single municipality. Each line shows the pace of implementation for a single municipality; bold black line is the national average. Source: Ministry of Education, 2016; CASEN, 2006-2017.

Figure 3.2: Expected JEC Exposure



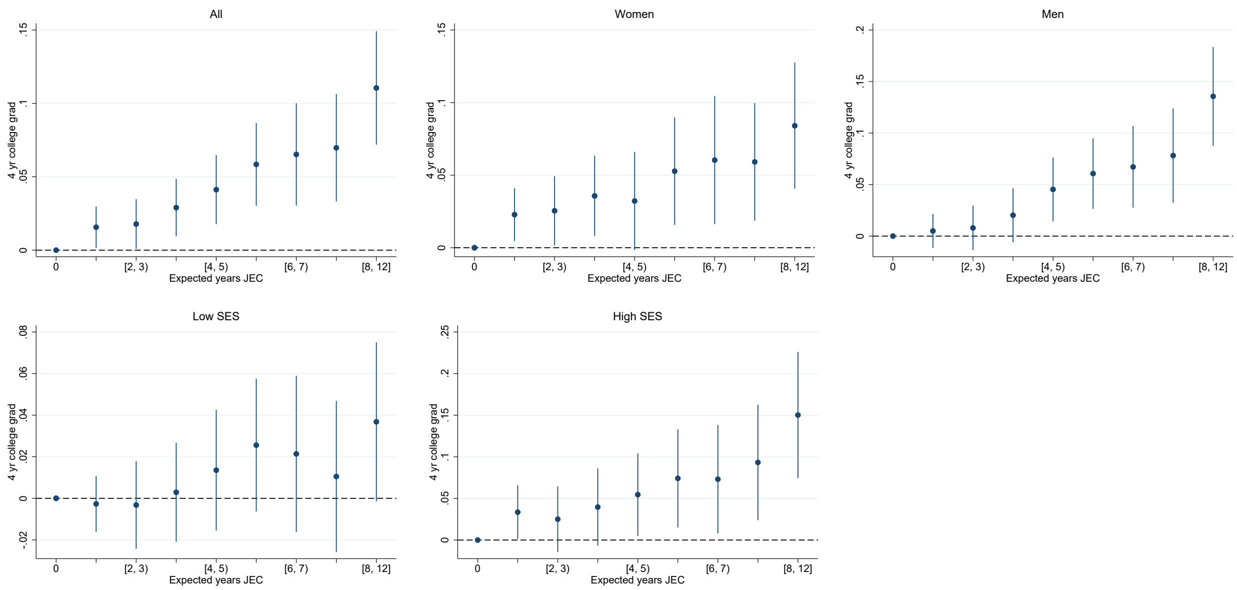
Notes: Figure shows the distribution of expected years of JEC attendance between grades 1-12 for our main sample of individuals born between 1979 and 1992 and who were 23-38 at the time of the CASEN survey and born outside the Santiago metropolitan region. The large mass point at exactly 0 years (11 percent of the sample) is omitted for visualization purposes. Source: Ministry of Education, 2016; CASEN, 2006-2017.

Figure 3.3: High School Graduation by Expected Years of Full-day Schooling



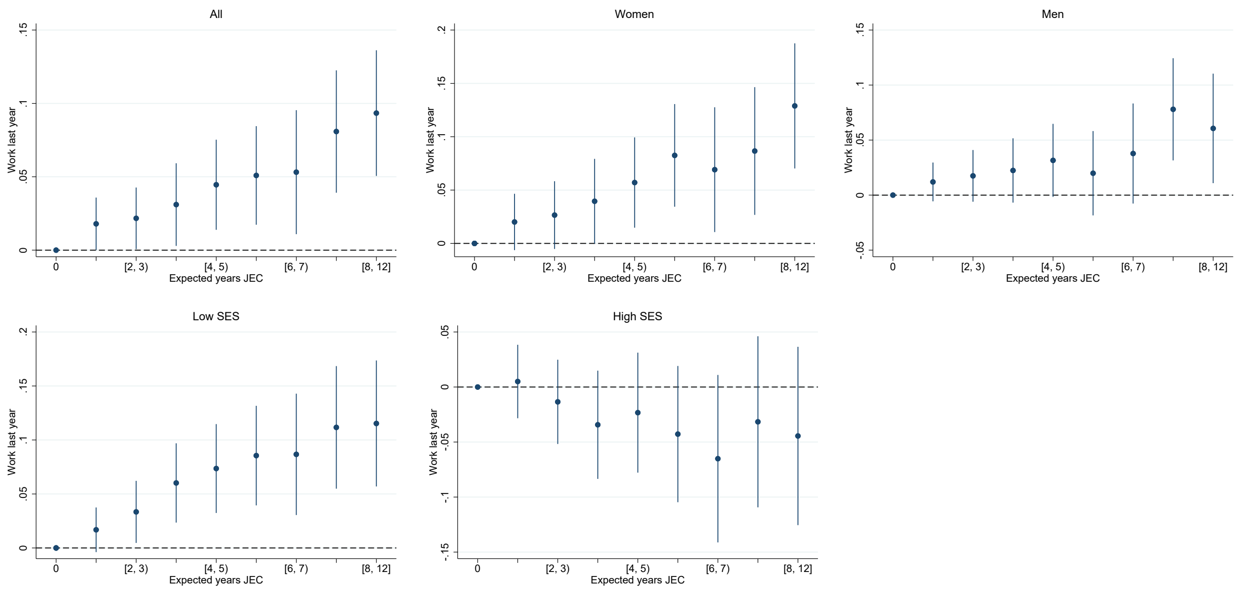
Notes: Figure shows the results from Equation 3.4 where each coefficient is one of nine indicators for 0, (0,1), [1,2)...[4,5), [8, 12] years of $\widehat{JEC}$. Dependent variable = 1 if the respondent had completed high school. All specifications include city of birth, survey year, and birth year-by-region fixed effects. Control variables include current municipality of residence employment and poverty rates, gender, a quadratic in age, indigenous identity, and maternal education, as well as survey year linear trends in baseline poverty and employment rates by municipality of birth from the 1996 CASEN. Vertical lines denote 95 percent confidence intervals clustered by city of birth. Sample limited to individuals born between 1979 and 1992 outside Santiago who were 19-38 years old at the time of survey. See text for details.

Figure 3.4: College Graduation by Expected Years of Full-day Schooling



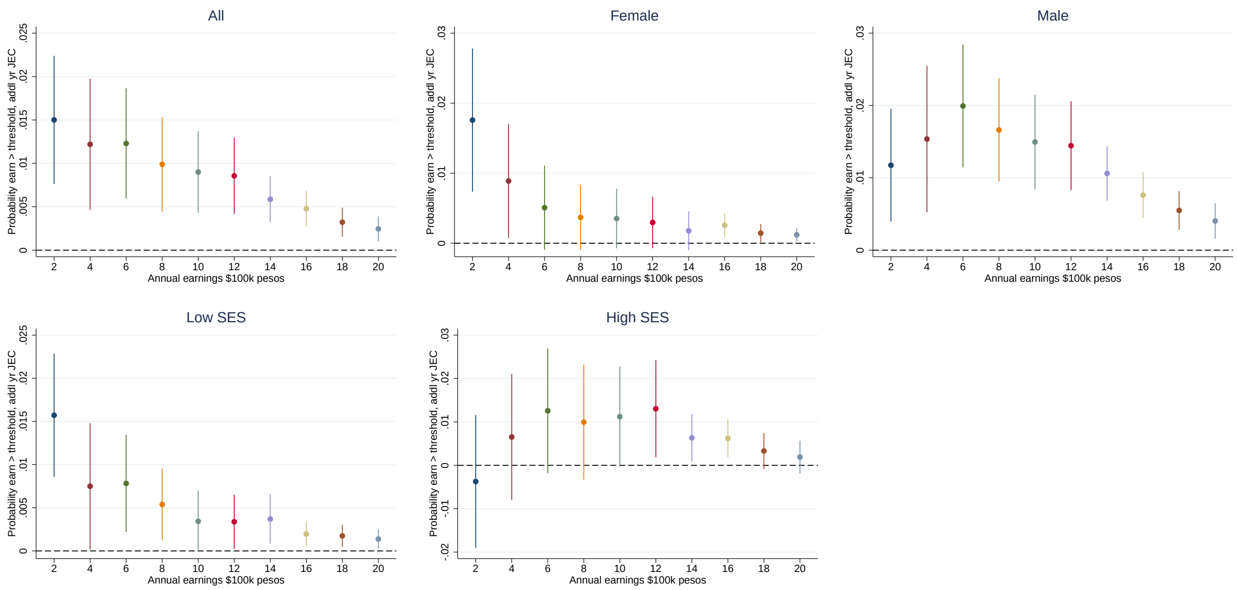
Notes: *Notes*: Figure shows the results from a regression as in Equation 3.4 where each coefficient is one of nine indicators for 0, (0,1), [1,2)...[4,5), [8, 12] years of $\widehat{JEC}$. Dependent variable is an indicator = 1 if the respondent had received a university degree at the time of the CASEN survey. All specifications include city of birth, survey year, and birth year-by-region fixed effects. Control variables include current municipality of residence employment and poverty rates, gender, a quadratic in age, indigenous identity, and maternal education, as well as survey year linear trends in baseline poverty and employment rates by municipality of birth from the 1996 CASEN. Vertical lines denote 95 percent confidence intervals clustered by city of birth. Sample limited to individuals born between 1979 and 1992 outside the Santiago metropolitan region who were 23-38 years old at the time of survey. See text for details.

Figure 3.5: Employment in Previous Month by Expected Years of Full-day Schooling



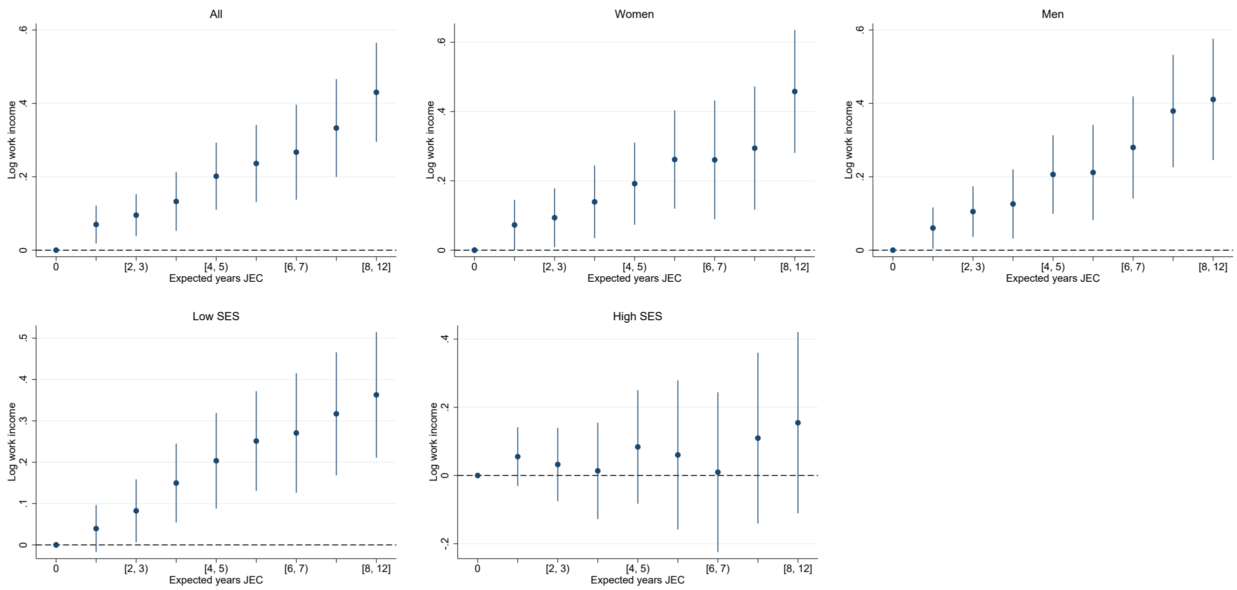
Notes: Figure shows the results from a regression as in Equation 3.4 where each coefficient is one of nine indicators for 0, (0,1), [1,2)...[4,5), [8, 12] years of $\widehat{JEC}$. Dependent variable is defined as having income at least 30,000 pesos (approximately $50) in the past month. All specifications include city of birth, survey year, and birth year-by-region fixed effects. Control variables include current municipality of residence employment and poverty rates, gender, a quadratic in age, indigenous identity, household size, maternal education, marital status and number and presence of children, interacted with gender, as well as linear survey year trends in baseline poverty and employment rates by municipality of birth from the 1996 CASEN. Vertical lines denote 95 percent confidence intervals clustered by city of birth. Sample limited to individuals born between 1979 and 1992 outside the Santiago metropolitan region who were 23-38 years old at the time of survey. See text for details.

Figure 3.6: Effects of JEC Across the Earnings Distribution



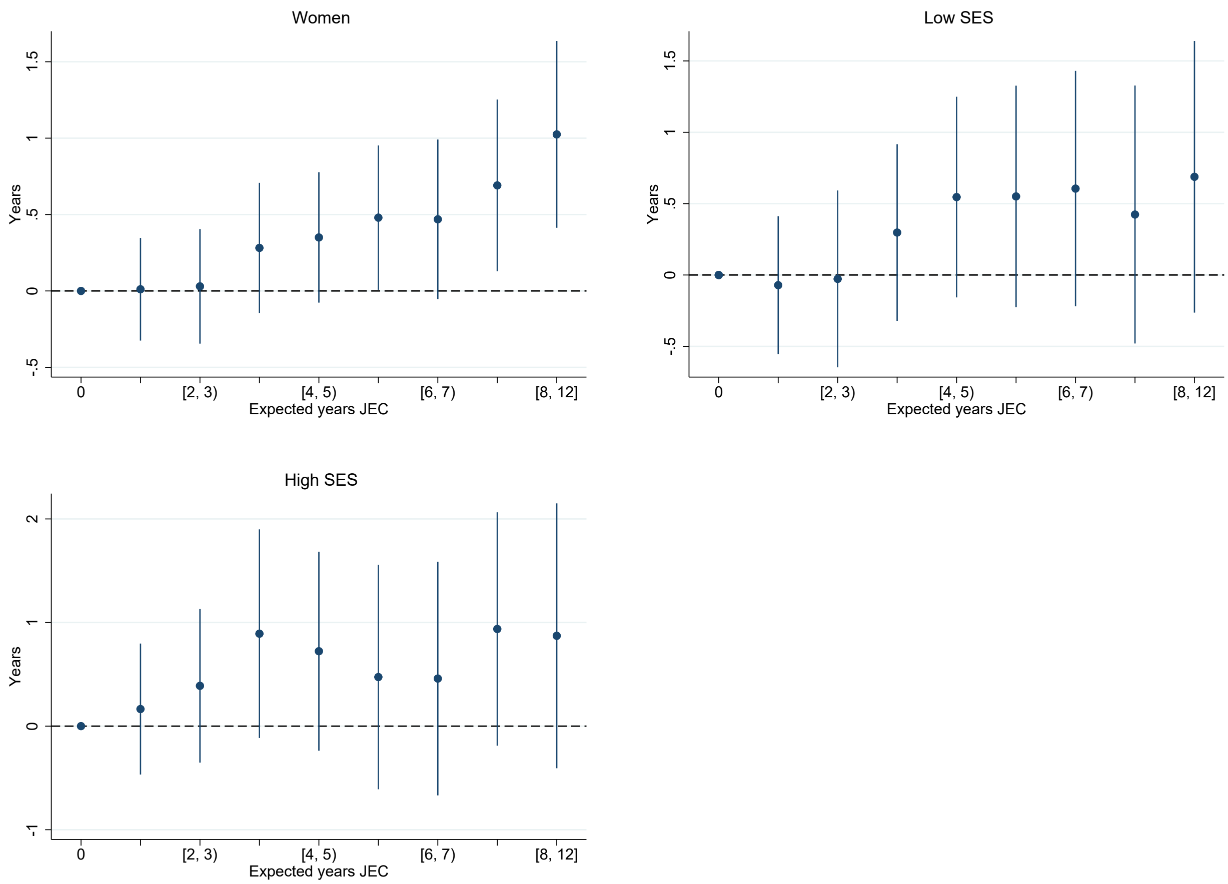
Notes: Figure shows the results from a series of regressions as in Equation 3.3 where the dependent variable is an indicator for whether monthly earnings were above a given threshold in 100,000s of 2017 pesos. All specifications include city of birth, survey year, and birth year-by-region fixed effects. Control variables include current municipality of residence employment and poverty rates, gender, a quadratic in age, indigenous identity, household size, maternal education, marital status and number and presence of children, interacted with gender, as well as linear survey year trends in baseline poverty and employment rates by municipality of birth from the 1996 CASEN. $\widehat{JEC}$ defined as the expected years of full-day school attendance based on an individual's city and year of birth, calculated as described in Equation 3.1 from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2006-2017 CASEN surveys. Vertical lines denote 95 percent confidence intervals clustered by city of birth. Sample limited to individuals born between 1979 and 1992 outside the Santiago metropolitan region who were 23-38 years old at the time of survey. See text for details.

Figure 3.7: Log Monthly Earnings by Expected Years of Full-day Schooling



Notes: Figure shows the results from a regression as in Equation 3.4 where each coefficient is one of nine indicators for 0, (0,1), [1,2)...[4,5), [8, 12] years of $\widehat{JEC}$. Dependent variable is $log(monthlyearnings + 1)$ (in 2017 pesos) in order to account for individuals with no earnings. All specifications include city of birth, survey year, and birth year-by-region fixed effects. Control variables include current municipality of residence employment and poverty rates, gender, a quadratic in age, indigenous identity, household size, maternal education, marital status and number and presence of children, interacted with gender, as well as linear survey year trends in baseline poverty and employment rates by municipality of birth from the 1996 CASEN. Vertical lines denote 95 percent confidence intervals clustered by city of birth. Sample limited to individuals born between 1979 and 1992 outside the Santiago metropolitan region who were 23-38 years old at the time of survey. See text for details.

Figure 3.8: Age at First Birth by Expected Years of Full-day Schooling



Notes: Figure shows the results from a regression as in Equation 3.4 where each coefficient is one of nine indicators for 0, (0,1), [1,2)...[4,5), [8, 12] years of $\widehat{JEC}$. Dependent variable is the age in years a woman gave birth to her first child. All specifications include city of birth, survey year, and birth year-by-region fixed effects. Control variables include current municipality of residence employment and poverty rates, gender, a quadratic in age, indigenous identity, and maternal education, as well as survey year linear trends in baseline poverty and employment rates by municipality of birth from the 1996 CASEN. Vertical lines denote 95 percent confidence intervals clustered by city of birth. Sample limited to women born between 1979 and 1992 outside the Santiago metropolitan region who had given birth to at least one child at the time of the survey. See text for details.

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Appendix A

Worker Earnings, Service Quality, and Firm Profits: Evidence from Nursing Homes and Minimum Wage Reforms

A.1 Tables and Figures

Table A.1: Average Nursing Home Prices, by Payment Source (2016)

	Private payor	Medicare	Medicaid
Alabama	200	343	196
Alaska	800	427	148
Arizona	210	457	66
Arkansas	170	348	236
California	267	565	115
Colorado	252	438	134
Connecticut	412	506	338
Delaware	350	433	275
DC	325	428	369
Florida	260	407	175
Georgia	205	368	129
Hawaii	376	504	207
Idaho	243	377	132
Illinois	187	430	99
Indiana	220	390	349
Iowa	189	360	217
Kansas	183	360	146
Kentucky	221	359	217
Louisiana	170	321	213
Maine	301	401	231
Maryland	300	426	199
Massachusetts	385	493	253

Table A.1: (continued))

	Private payor	Medicare	Medicaid
Michigan	262	404	176
Minnesota	269	427	165
Mississippi	214	333	254
Missouri	162	358	184
Montana	237	365	160
Nebraska	208	368	180
Nevada	240	484	79
New Hampshire	318	421	258
New Jersey	330	516	188
New Mexico	209	398	131
New York	364	509	409
North Carolina	225	379	138
North Dakota	350	278	318
Ohio	224	396	244
Oklahoma	147	344	142
Oregon	289	495	101
Pennsylvania	305	406	318
Rhode Island	279	440	345
South Carolina	211	365	122
South Dakota	207	325	166
Tennessee	201	355	138
Texas	150	394	109
Utah	185	439	64
Vermont	288	411	197
Virginia	235	392	115
Washington	282	488	87
West Virginia	319	337	348
Wisconsin	260	406	167
Wyoming	233	374	194
US average (unweighted)	263	407	195

Notes: Table shows average per-day cost of nursing home care, by payment source. Private and Medicaid rates from AARP (2018); Medicare rates author's calculations from Centers for Medicare and Medicaid Services (2018).

Table A.2: Descriptive Statistics, All Facilities and County Pair Sample

	(1)	(2)
	All counties	County pair
Panel a: County staffing		
Min wage (2017$)	7.532	7.600
	(0.789)	(0.856)
$\leq$ HS female employment	385.7	519.3
	(675.7)	(1064.1)
$\leq$ HS female earnings	1861.5	1946.3
	(345.2)	(372.8)
$\leq$ HS female turnover	0.236	0.224
	(0.173)	(0.254)
Observations	205735	170578
Panel b: Facility staffing		
Min wage (2017$)	7.948	8.280
	(0.875)	(1.087)
NA hrs per resident day	2.268	2.308
	(0.671)	(0.670)
LPN hrs per resident day	0.810	0.799
	(0.351)	(0.379)
RN hrs per resident day	0.449	0.504
	(0.419)	(0.446)
FTE aides	36.74	37.88
	(24.11)	(25.75)
FTE LPNs	12.90	12.92
	(8.993)	(9.799)
FTE RNs	6.420	7.454
	(5.679)	(6.779)
Observations	907550	991486

Table A.2: (continued)

	(1)	(2)
	All counties	County pair
Panel c: Inspection violations		
Min wage (2017$)	7.818	8.190
	(0.925)	(1.124)
Any violation	0.931	0.933
	(0.254)	(0.250)
# violations	5.795	6.254
	(4.895)	(5.282)
Any severe violation	0.175	0.167
	(0.380)	(0.373)
# severe violations	0.298	0.282
	(0.859)	(0.836)
Standardized score	0.000	0.000
	(1.000)	(1.000)
Any care violation	0.841	0.846
	(0.366)	(0.361)
# care violations	3.284	3.430
	(3.075)	(3.046)
Any severe care violation	0.143	0.138
	(0.350)	(0.345)
# severe care violations	0.183	0.176
	(0.501)	(0.497)
Standardized care score	0.000	0.005
	(1.000)	(0.992)
Observations	316244	360591

Table A.2: (continued)

Panel d: Patient health		
Min wage (2017$)	7.940	8.283
	(0.899)	(1.118)
% pressure ulcers	0.089	0.095
	(0.059)	(0.061)
% UTI	0.076	0.076
	(0.050)	(0.050)
% restraints	0.030	0.033
	(0.045)	(0.049)
% psychotropic medication	0.195	0.191
	(0.097)	(0.096)
Observations	997909	1092186
Panel e: Mortality		
Age-adjusted mortality (all)	0.052	0.052
	(0.020)	(0.018)
Age-adjusted mortality (nursing home)	0.037	0.037
	(0.019)	(0.017)
Age-adjusted mortality (non-nursing home)	0.016	0.015
	(0.009)	(0.008)
Age-adjusted mortality (hospital)	0.019	0.020
	(0.018)	(0.016)
Observations	71319	58182

Table A.2: (continued)

	(1)	(2)
	All counties	County pair
Panel f: Controls (patient health sample)		
County unemployment (x100)	6.284	6.490
	(2.651)	(2.618)
% population > 65	15.000	14.200
	(3.990)	(3.340)
State EITC rate (x100)	0.072	0.087
	(0.133)	(0.168)
Any state EITC	0.435	0.437
	(0.496)	(0.496)
TANF/AFDC maximum	486.0	552.9
	(196.9)	(210.3)
Avg facility size	106.2	107.0
	(62.89)	(65.59)
CZ HHI	588.9	460.0
	(990.2)	(871.4)
% NH residents female	69.49	68.72
	(11.59)	(12.08)
% NH residents black	15.00	17.40
	(22.09)	(23.71)
% NH residents Medicaid	59.68	59.25
	(23.44)	(24.64)
Avg NH resident age	80.57	80.27
	(5.892)	(6.298)
Observations	997909	1092186

Notes: Table shows average characteristics (standard deviations in parentheses) for all facilities (column (1)) and facilities in the county-pairs sample (column (2)) for QWI county-level employment (a), OSCAR/CASPER facility-level staffing measures (b), inspection violations (c), patient health outcomes (d), mortality (e), and area economic and policy, as well as facility demographic controls (f). "County" pairs sample consists of facilities located in a county that border another county with a different minimum wage at any point over the 1991 through 2017 period. See text for details.

Table A.3: Alternative Measures of Nursing Home Employment

	(1)	(2)	(3)	(4)	(5)	(6)
	Nursing assistant		Vocational nurse		Registered nurse	
Panel a: Log number full-time employees						
log(MW)	0.0554**	0.0700***	0.0110	0.0090	-0.0617	-0.0719*
	(0.0252)	(0.0254)	(0.0313)	(0.0317)	(0.0387)	(0.0396)
Observations	402697	402697	395721	395721	383506	383506
DV mean (level)	32.080	32.080	10.830	10.830	6.212	6.212
Panel b: Log number part-time employees						
log(MW)	0.1930***	0.1936***	0.0162	0.0093	0.1918***	0.1842***
	(0.0553)	(0.0553)	(0.0650)	(0.0650)	(0.0532)	(0.0530)
Observations	313592	313592	295538	295538	290793	290793
DV mean (level)	7.458	7.458	2.785	2.785	2.105	2.105
Demographic controls		X		X		X

Notes: Table shows staffing results from the OSCAR/CASPER staffing reports reported by facilities to CMS, covering years 1992-2017. Sample includes facilities in counties that straddle a minimum wage discontinuity. $log(MW)$ is defined as the natural log of the county minimum wage at time t in 2017 dollars, adjusted for inflation using the CPI-U-RS. Log number total employees is defined as the (natural log) of employees working at least 35 hours a week (full-time) or fewer than 35 hours a week (part-time) for each occupation group. All specifications include county-pair-time and facility fixed effects and controls for county employment rates and the elderly population share; and state EITC parameters, the share of the elderly population receiving Supplemental Security Income, and AFDC/TANF caseloads and benefit levels. Odd-numbered columns do not include controls for facility patient mix; even-numbered columns include facility market concentration and demographic controls: average resident age, and the share of residents female, white, black, and covered by Medicaid. Robust standard errors clustered by county. All regressions weighted by facility size. See text for details. *** $= p < 0.01$, ** $= p < 0.05$, * $= p < 0.10$.

Table A.4: Alternative Samples: Nursing Home Employment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		Nursing assistants			LPN/LVNs			RNs	
	Panel a: Log hours per resident day								
log(MW)	-0.0167	0.0109	-0.0065	0.0218	0.0036	0.0002	-0.1169***	-0.0883**	-0.1205***
	(0.0193)	(0.0180)	(0.0187)	(0.0246)	(0.0223)	(0.0234)	(0.0412)	(0.0367)	(0.0420)
Observations	272691	269059	254694	270214	266653	253413	271357	267922	254291
DV mean (level)	2.3613	2.2705	2.2513	0.8489	0.7857	0.7695	0.5536	0.4842	0.4447
	Panel b: Log number total employees								
log(MW)	0.0157	0.0045	0.0852***	-0.0248	-0.0259	0.0425	-0.0890**	-0.1113***	-0.0304
	(0.0320)	(0.0216)	(0.0240)	(0.0409)	(0.0244)	(0.0278)	(0.0441)	(0.0292)	(0.0356)
Observations	410344	412547	384252	409080	412700	383180	414051	417620	387011
DV mean (level)	38.5537	37.9312	38.7209	12.8065	12.7178	12.8955	7.4487	7.4252	7.3739
Outliers	X			X			X		
Hospitals	X	X		X	X		X	X	
Weight	# residents	Unweighted	# residents	# residents	Unweighted	# residents	# residents	Unweighted	# residents

Notes: Table shows staffing results from the OSCAR/CASPER staffing reports reported by facilities to CMS, covering years 2000-2016 (panel a) and 1992-2017 (panel b). Sample includes facilities in counties that straddle a minimum wage discontinuity. “Outlier” specifications (columns (1), (4), and (7)) include facilities with reported employment above the 99th percentile; columns (2), (5), and (8) replace resident weights with unweighted specifications; columns (3), (6), and (9) exclude facilities located in a hospital. $log(MW)$ is defined as the natural log of the county minimum wage at time t in 2017 dollars, adjusted for inflation using the CPI-U-RS. Log hours per resident day is defined as the number of staffing hours for each occupation divided by the number of residents. Log number total employees is defined as the (natural log) of full-time equivalent workers for each occupation group. All specifications include county-pair-time and facility fixed effects and controls for county employment rates and the elderly population share; state EITC parameters, the share of the elderly population receiving Supplemental Security Income, and AFDC/TANF caseloads and benefit levels; and facility market concentration, average resident age, and the share of residents female, white, black, and covered by Medicaid. Robust standard errors clustered by county. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table A.5: Full County Sample: Nursing Assistant Employment

	(1)	(2)	(3)	(4)
	Nursing assistants			
	Panel a: Log hours per resident day			
log(MW)	0.091***	0.082***	0.081***	0.081***
	(0.019)	(0.018)	(0.018)	(0.020)
Observations	256112	256112	256112	256112
DV mean (level)	2.130	2.130	2.130	2.130
	Panel b: Log number total employees			
log(MW)	0.157***	0.036*	0.058**	0.064***
	(0.028)	(0.021)	(0.025)	(0.021)
Observations	362047	362047	362047	362047
DV mean (level)	28.905	28.905	28.905	28.905
Division X year FE		X		X
State trends			X	X

Notes: Table shows staffing results from estimating Equation 1.3 on the OSCAR/CASPER staffing reports reported by facilities to CMS, covering years 2000-2016 (panel a) and 1992-2017 (panel b). Sample includes all nursing home facilities. $log(MW)$ is defined as the natural log of the minimum wage faced by facility f at time t in 2017 dollars, adjusted for inflation using the CPI-U-RS. All specifications include facility fixed effects and controls for county employment rates and the elderly population share; state EITC parameters, the share of the elderly population receiving Supplemental Security Income, and AFDC/TANF caseloads and benefit levels; and facility average resident age, market concentration, and the share of residents female, white, black, and covered by Medicaid. Columns (2) through (4) also include division-by-year and/or state linear trends. Robust standard errors clustered by county. All regressions weighted by facility size. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table A.6: Worker Illness and Injury

	(1)	(2)	(3)
	TCR	DART	DAFWII
log(MW)	-2.747***	-0.802	-0.089
	(0.916)	(0.584)	(0.376)
Observations	76218	76218	76218
DV mean	10.959	7.041	3.555
ϵ_{mw}	-0.251	-0.114	-0.025
Cty controls	X	X	X
Division X time FE	X	X	X
State linear trends	X	X	X

Notes: Table shows staffing results from estimating Equation 1.3 on establishment specific injury and illness data from the Department of Labor's Occupational Safety and Health Administration (OSHA) for years 2002 through 2011. Sample includes all surveyed nursing home facilities, identified as those with NAICS code 6231 or SIC codes 8052, 8059, 8062, 8082, or 8361 in order to maximize comparability across years. $log(MW)$ is defined as the natural log of the minimum wage faced by facility f at time t in 2017 dollars, adjusted for inflation using the CPI-U-RS. Column (1) shows the Total Case Rate (TCR), defined as (Number of OSHA Recordable injuries and illnesses X 200,000) / Employee total hours worked. Column (2) show the Days Away Restricted Transfer (DART) rate, defined as Days away, restricted, transferred*200,000)/employee hours worked. Column (3) shows the Days Away With Illness or Injury (DAFWII) rate, defined as (Days away with illness or injury*200,000)/employee hours worked. All specifications include Census Division-time and county fixed effects, state linear trends, and controls for county employment rates and the elderly population share; state EITC parameters, the share of the elderly population receiving Supplemental Security Income, and AFDC/TANF caseloads and benefit levels. Robust standard errors clustered by county. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table A.7: Share of Nursing Home Violations by Severity and Scope, 2017

	Scope		
Severity	Isolated	Pattern	Widespread
Potential for minimal harm		2.23%	1.81%
Potential for > minimal harm	60.28%	25.38%	5.37%
Actual harm	3.20%	0.22%	0.02%
Immediate jeopardy	0.73%	0.56%	0.19%

Notes: Table shows the share of nursing home inspection violations in 2017 by severity (rows) and scope (columns). See text for details.

Table A.8: Alternative Samples: Health Inspection Violations and Patient Safety

	(1)	(2)	(3)	(4)	(5)	(6)
	All violations			Quality of care violations		
	#	# severe	Std score	#	# severe	Std score
			Panel a: Including outliers			
log(MW)	-1.0015**	0.0602	-0.1896**	-0.7612***	0.0368	-0.0560
	(0.4590)	(0.0753)	(0.0869)	(0.2247)	(0.0465)	(0.0661)
N	348731	348731	348731	348731	348731	348731
DV mean	6.6877	0.3628	0.0820	3.6514	0.2027	0.0239
ϵ_{mw}	-0.1498	0.1659		-0.2085	0.1815	
			Panel b: Unweighted			
log(MW)	-0.4048	0.0907*	-0.0766	-0.6339***	0.0445	-0.0592
	(0.2944)	(0.0492)	(0.0557)	(0.1726)	(0.0279)	(0.0468)
N	354960	354960	354960	354960	354960	354960
DV mean	6.2587	0.3224	0.0008	3.4357	0.1820	-0.0330
ϵ_{mw}	-0.0647	0.2813		-0.1845	0.2445	
			Panel c: Excluding facilities in hospitals			
log(MW)	-0.4581	0.0882	-0.0867	-0.6945***	0.0685*	-0.0682
	(0.3389)	(0.0608)	(0.0642)	(0.1941)	(0.0360)	(0.0534)
N	328703	328703	328703	328703	328703	328703
DV mean	6.4589	0.3353	0.0387	3.5391	0.1895	-0.0112
ϵ_{mw}	-0.0709	0.2630		-0.1962	0.3615	

Table A.8: (continued)

	(1)	(2)	(3)	(4)	(5)	(6)
	All violations			Quality of care violations		
	#	# severe	Std score	#	# severe	Std score
Panel d: Hospital Referral Region						
log(MW)	-0.363	-0.0103	0.0823	-0.382*	-0.0001	0.0261
	(0.363)	(0.0834)	(0.0703)	(0.223)	(0.0435)	(0.0712)
N	223933	223933	223933	223933	223933	223933
DV mean	6.0270	0.3440	-0.0342	3.3250	0.1920	-0.0077
ϵ_{mw}	-0.0602	-0.0299		-0.1149	-0.0005	

Notes: Table shows staffing results from facility health inspections for 1998-2017. Sample includes facilities in counties that straddle a minimum wage discontinuity. Panel (a) include facilities with violations above the 99th percentile; panel (b) replaces resident weights with unweighted specifications; panel (c) excludes facilities located in a hospital; and panel (d) replaces the county-pair sample with facilities where the minimum wage differs within a Hospital Referral Regions (HRR). $log(MW)$ is defined as the natural log of the minimum wage faced by facility f at time t in 2017 dollars, adjusted for inflation using the CPI-U-RS. "Severe" violations present actual harm or immediate jeopardy to residents (CMS categories G-L). "Quality of care" violations follow the definition in Harrington et al. (2001). "Standardized score" is a normalized measure of the CMS-issued score (Centers for Medicare and Medicaid Services, 2011). All specifications include facility fixed effects and controls for county employment rates and the elderly population share; state EITC parameters, the share of the elderly population receiving Supplemental Security Income, and AFDC/TANF caseloads and benefit levels; and the facility share of residents female, white, black, and covered by Medicaid. Panels (a) through (c) include county-pair-year fixed effects; panel (d) includes HRR-year fixed effects. Robust standard errors clustered by county. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table A.9: Full County Sample: Health Inspection Violations and Patient Safety

	(1)	(2)	(3)	(4)	(5)	(6)
	#		# severe		Standardized score	
	Panel a: All inspection violations					
log(MW)	-1.676***	-1.351***	-1.420***	-0.036	-0.342***	-0.275***
	(0.528)	(0.504)	(0.075)	(0.078)	(0.108)	(0.103)
N	307025	307025	307025	307025	307025	307025
DV mean	6.162	6.162	0.374	0.374	0.0748	0.0748
ϵ_{mw}	-0.272	-0.219	-3.797	-0.097		
	Panel b: Quality of care violations					
log(MW)	-1.057***	-0.692***	-0.0346	-0.0066	-0.179**	-0.061
	(0.271)	(0.248)	(0.0424)	(0.0448)	(0.073)	(0.071)
N	307025	307025	307025	307025	307025	307025
DV mean	3.367	3.367	0.206	0.206	0.0177	0.0177
ϵ_{mw}	-0.314	-0.206	-0.168	-0.032		
Division X year FE	X	X	X	X	X	X
State linear trends		X		X		X

Notes: Table shows staffing results from estimating Equation 1.3 on the state health inspection reports reported to CMS, covering years 1998-2017. Sample includes all nursing home facilities. $log(MW)$ is defined as the natural log of the minimum wage faced by facility f at time t in 2017 dollars, adjusted for inflation using the CPI-U-RS. "Severe" violations are those presenting actual harm or immediate jeopardy to residents (CMS categories G-L). "Quality of care" violations follow the definition in Harrington et al. (2001) to include violations in the quality of care, assessment, nursing, dietary, physician, rehabilitative services, dental, and pharmacy regulation categories. "Standardized score" allocates violation points to each violation based on the CMS scoring criteria and normalizes the score distribution across facilities (Centers for Medicare and Medicaid Services, 2011). All specifications include Census Division-time and facility fixed effects and controls for county employment rates and the elderly population share; state EITC parameters, the share of the elderly population receiving Supplemental Security Income, and AFDC/TANF caseloads and benefit levels; and facility average resident age, market concentration, and the share of residents female, white, black, and covered by Medicaid. Columns (2), (4), and (6) also include state linear trends. Robust standard errors clustered by county. All regressions weighted by facility size. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table A.10: Alternative Samples: Patient Health

	(1)	(2)	(3)	(4)	(5)
	Share sores	Share UTI	Share restraint	Share psychotropic	Health index
Panel a: With outlier observations					
log(MW)	-0.0165***	-0.0074	-0.0131	0.0346	-0.2397**
	(0.0057)	(0.0051)	(0.0112)	(0.0292)	(0.1000)
N	292587	332334	332800	180639	292555
DV mean	0.0858	0.0741	0.0273	0.1939	-0.0922
Δ # residents (1000s), 10% increase	-2.224	-0.997	-1.765	4.663	
ϵ_{mw}	-0.1923	-0.0999	-0.4799	0.1784	
Panel b: Unweighted					
log(MW)	-0.0127***	-0.0086*	-0.0061	0.0281	-0.2062***
	(0.0046)	(0.0045)	(0.0039)	(0.0275)	(0.0597)
N	289855	329915	330077	179169	286092
DV mean	0.0833	0.0726	0.0249	0.1901	-0.1035
Δ # residents (1000s), 10% increase	-1.711	-1.159	-0.822	3.787	
ϵ_{mw}	-0.1525	-0.1185	-0.2450	0.1478	
Panel c: Excluding facilities in hospitals					
log(MW)	-0.0141***	-0.0071	-0.0076	0.0338	-0.2021***
	(0.0052)	(0.0051)	(0.0050)	(0.0295)	(0.0738)
N	280797	319169	319088	172449	277100
DV mean	0.0834	0.0726	0.0252	0.1910	-0.1004
Δ # residents (1000s), 10% increase	-1.900	-0.957	-1.024	4.555	
ϵ_{mw}	-0.1691	-0.0978	-0.3016	0.1770	

Table A.10: (continued)

	(1)	(2)	(3)	(4)	(5)
	Share sores	Share UTI	Share restraint	Share psychotropic	Health index
Panel d: Hospital Referral Region					
log(MW)	-0.0117***	-0.0097*	-0.0068*	-0.0033	-0.1766***
	(0.0045)	(0.0053)	(0.0039)	(0.0148)	(0.0594)
N	150144	170080	170711	100609	148762
DV mean	0.0757	0.0715	0.0209	0.1890	-0.1858
Δ # residents (1000s), 10% increase	-1.577	-1.307	-0.916	-0.445	
ϵ_{mw}	-0.1546	-0.1357	-0.3254	-0.0175	

Notes: Table shows patient outcomes results from long-term resident assessment reports reported by facilities to CMS, covering years 2005-2017. Sample includes facilities in counties that straddle a minimum wage discontinuity. Panel (a) includes facilities with reported employment above the 99th percentile; panel (b) replaces resident weights with unweighted specifications. Panel (c) excludes facilities located within hospitals. Panel (d) replaces the county-pair sample with facilities where the minimum wage differs within a Hospital Referral Regions (HRR). $log(MW)$ is defined as the natural log of the minimum wage faced by facility f at time t in 2017 dollars, adjusted for inflation using the CPI-U-RS. All specifications include facility fixed effects and controls for county employment rates and the elderly population share; state EITC parameters, the share of the elderly population receiving Supplemental Security Income, and AFDC/TANF caseloads and benefit levels; and facility average resident age, market concentration, and the share of residents female, white, black, and covered by Medicaid. Panels (a) through (c) include county-pair-year fixed effects; panel (d) includes HRR-year fixed effects. Robust standard errors clustered by county. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table A.11: Full County Sample: Patient Health

	(1)	(2)	(3)	(4)	(5)
	Share sores	Share UTI	Share restraint	Share psychotropic	Health index
Panel a: Division X year FE					
log(MW)	-0.012***	-0.011***	-0.005*	0.013	-0.195***
	(0.003)	(0.004)	(0.003)	(0.010)	(0.043)
N	299209	336236	337556	199272	296266
DV mean	0.080	0.071	0.022	0.193	-0.164
Δ # residents (1000s), 10% increase	-1.64	-1.54	-0.67	1.79	
ϵ_{mw}	-0.154	-0.161	-0.228	0.069	
Panel b: Divison X year FE and state linear trends					
log(MW)	-0.013***	-0.011***	-0.008***	0.001	-0.228***
	(0.003)	(0.003)	(0.002)	(0.011)	(0.0364)
N	299209	336236	337556	199272	296266
DV mean	0.079	0.071	0.022	0.193	-0.164
Δ # residents (1000s), 10% increase	-1.21	-1.23	-0.23	1.87	
ϵ_{mw}	-0.11	-0.13	-0.08	0.07	

Notes: Table shows patient outcomes results from estimating Equation 1.3 on long-term resident assessment reports reported by facilities to CMS, covering years 2005-2017. Sample includes all facilities. $log(MW)$ is defined as the natural log of the minimum wage faced by facility f at time t in 2017 dollars, adjusted for inflation using the CPI-U-RS. All specifications include Census Division-time and facility fixed effects and controls for county employment rates and the elderly population share; state EITC parameters, the share of the elderly population receiving Supplemental Security Income, and AFDC/TANF caseloads and benefit levels; and facility average resident age, facility market concentration, and the share of residents female, white, black, and covered by Medicaid. Panel (b) also includes state linear time trends. Robust standard errors clustered by county. All regressions weighted by facility size. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table A.12: Full County Sample: Mortality

	(1)	(2)	(3)
	Panel a: All deaths		
log(MW)	-0.0297	-0.0113	-0.0189
	(0.0215)	(0.0128)	(0.0287)
N	71052	71052	58351
DV mean (level)	0.052	0.052	0.051
	Panel b: Nursing home deaths		
log(MW)	-0.218***	0.189*	-0.286*
	(0.0448)	(0.110)	(0.157)
N	66895	66895	55382
DV mean (level)	0.017	0.017	0.017
Δ # residents (1000s), 10% increase	-10.630	9.216	-13.946
	Panel c: Non-nursing home deaths		
log(MW)	0.0768***	-0.0350	0.0397
	(0.0208)	(0.0325)	(0.0747)
N	71012	71012	58313
DV mean (level)	0.037	0.037	0.035
County controls	X	X	X
Division X year FE	X	X	
State linear trends		X	
HRR X year FE			X

Notes: Table shows changes in county-level age-adjusted mortality rates from estimating Equation 1.3 on county mortality counts by age and place of death covering years 1990-2013 for the population ages 65 and older. The age adjustment, defined in Equation 1.4, holds the age composition of the population fixed at its 2000 distribution; see (Stevens et al., 2015). Sample in columns (1) and (2) includes all counties. $log(MW)$ is defined as the natural log of the highest minimum wage in county c at time t in 2017 dollars, adjusted for inflation using the CPI-U-RS. All specifications control for county employment rates and the elderly population share as well as state EITC parameters, the share of the elderly population receiving SSI, and AFDC/TANF caseloads and benefit levels. Specifications in columns (1) and (2) include Census Division x year and fixed effects; column (3) includes counties where the minimum wage differs within the HRR and includes HRR-by-year fixed effects. Robust standard errors clustered by county. All regressions weighted by county elderly population. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table A.13: Predicted Changes in Patient Safety and Health

	(1)	(2)	(3)
	$QOC\widehat{violations}$	$Pressure\widehat{ulcers}$	$NH\widehat{Mortality}$
log(MW)	-0.0763**	-0.0017	-0.0233
	(0.0375)	(0.0012)	(0.0306)
N	58592	144844	23794
County controls	X	X	X
% main effect from	0.103	0.122	0.075

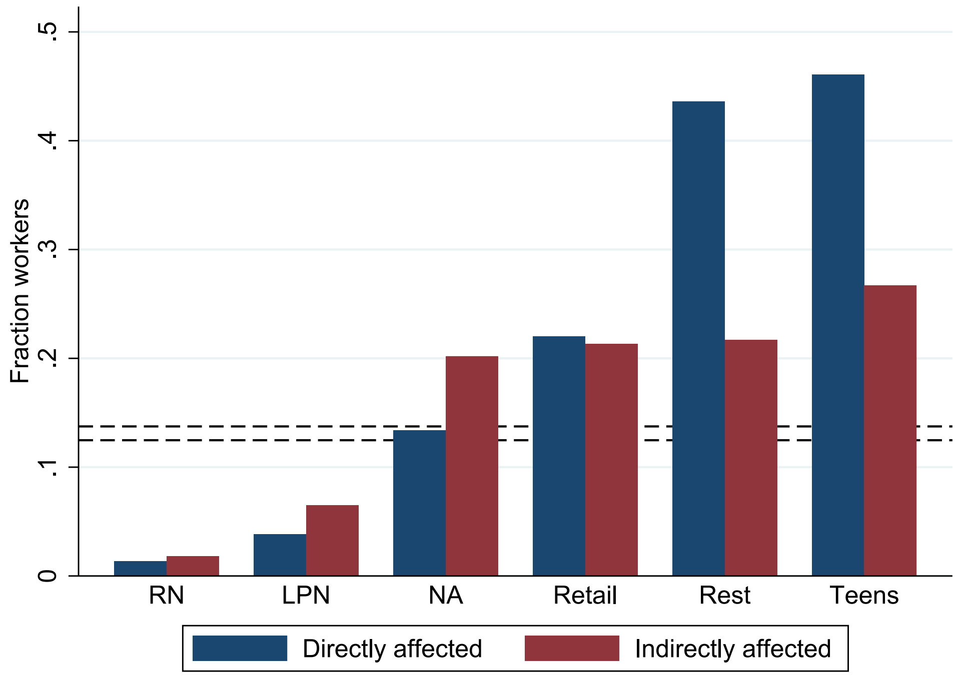
Notes: Table shows the expected number of quality-of-care violations (column (1)), fraction of residents with pressure ulcers (column (2)), fraction of residents with UTIs (column (3)), and county log mortality rate (column (4)) based on changes in resident demographic characteristics. Dependent variable estimated from a regression with cubic terms for the fraction of residents female, white, black, and covered by Medicaid, as well a cubic in average facility age. Sample includes facilities in counties that straddle a minimum wage discontinuity. $log(MW)$ is defined as the natural log of the minimum wage faced by facility f at time t in 2017 dollars, adjusted for inflation using the CPI-U-RS. All specifications include county-pair-time and facility fixed effects and controls for county employment rates and the elderly population share; and state EITC parameters, the share of the elderly population receiving Supplemental Security Income, and AFDC/TANF caseloads and benefit levels. Robust standard errors clustered by county. All regressions weighted by facility size. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table A.14: Staffing Changes by County Characteristics

	(1)	(2)	(3)	(4)	(5)
	High Medicaid	Private	Chain	Competitive	Direct care staff reqt
Panel a: Log (employment)					
log(MW)	-0.080	-0.114	-0.097	-0.123	-0.075
	(0.109)	(0.105)	(0.106)	(0.111)	(0.106)
log(MW) X char	-0.027*	0.027	0.010	0.025	-0.023
	(0.015)	(0.022)	(0.013)	(0.023)	(0.018)
N	25974	25974	25966	25974	25974
DV mean (level) | char $< p_{50}$	99.39	107.2	165.6	21.59	132.4
DV mean (level) | char $\geq p_{50}$	174.2	167.9	101.3	137.4	135.7
$\overline{char}$	0.469	0.712	0.560	0.977	0.720
Panel b: Log (earnings)					
log(MW)	0.114***	0.121***	0.118***	0.111***	0.122***
	(0.031)	(0.032)	(0.031)	(0.033)	(0.033)
log(MW) X char	0.004	-0.008	-0.009**	0.005	-0.007
	(0.004)	(0.007)	(0.004)	(0.007)	(0.007)
N	23044	23044	23036	23044	23044
DV mean (level) | char $< p_{50}$	2063.4	2129.6	2113.9	1802.1	1961.5
DV mean (level) | char $\geq p_{50}$	2117.3	2042.4	2063.2	2096.0	2138.7
$\overline{char}$	0.475	0.717	0.562	0.978	0.722

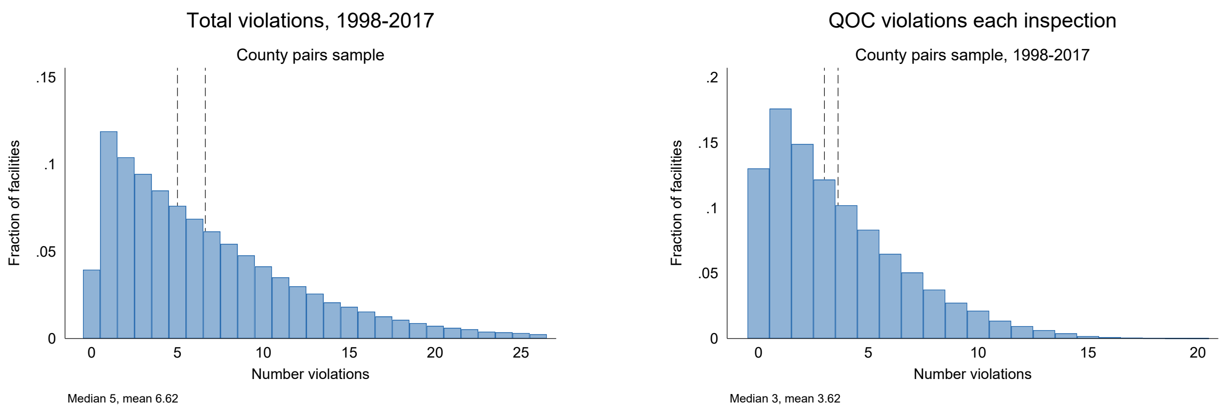
Notes: Table shows earnings and employment for workers with a high school education or less from the QWI. Sample includes counties that straddle a minimum wage discontinuity. $log(MW)$ is defined as the natural log of the minimum wage faced by facility f at time t in 2017 dollars, adjusted for inflation using the CPI-U-RS. $char$ interacts the (bed-weighted) share of facilities in a county satisfying each characteristic in the column header (high Medicaid share, private ownership, chain, located in a competitive industry, or in a state with a direct staffing requirement). All specifications include county-pair-time and county fixed effects and controls for county employment rates and the elderly population share; and state EITC parameters, the share of the elderly population receiving Supplemental Security Income, and AFDC/TANF caseloads and benefit levels. All regressions weighted by county population. Robust standard errors clustered by county. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Figure A.1: Share of Workers Affected by 10% Minimum Wage Increase



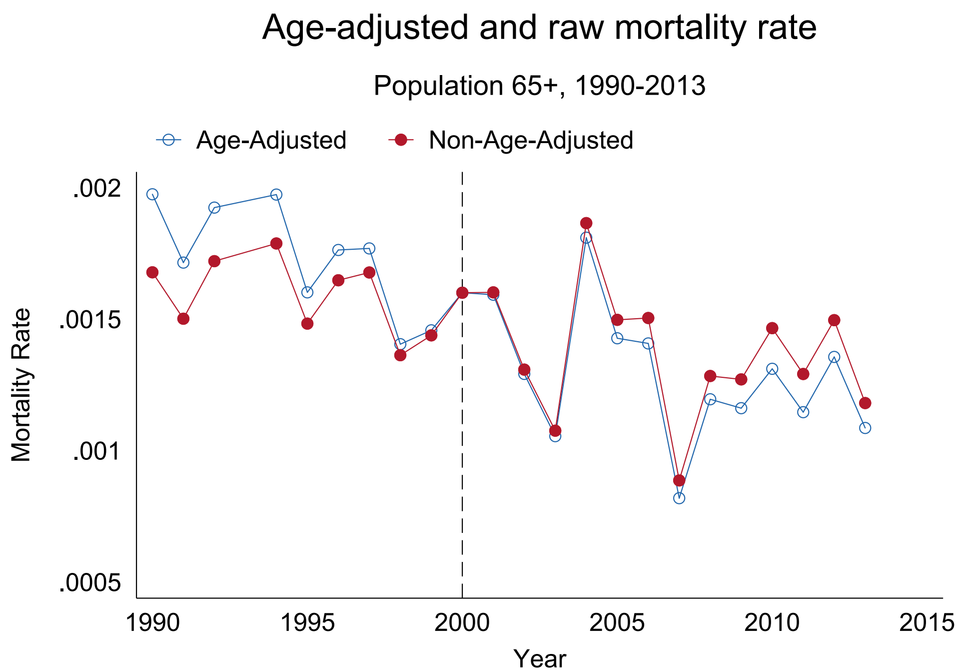
Notes: Figure shows the share of workers earning less than 110 percent of the current minimum wage ("directly affected") and those earning between 110 percent and 126.5 percent (115 percent of 110 percent) of the minimum wage ("indirectly affected") by industry and occupation. Horizonal lines denote the share of all private sector workers directly and indirectly affected (13.7 and 12.5 percent, respectively). Hourly wages calculated as hourly wage for those paid hourly and weekly earnings divided by usual hours worked for those not reporting hourly wage. See text for details. Source: CPS MORG 2014-2018.

Figure A.2: Number of Health Inspection Violations



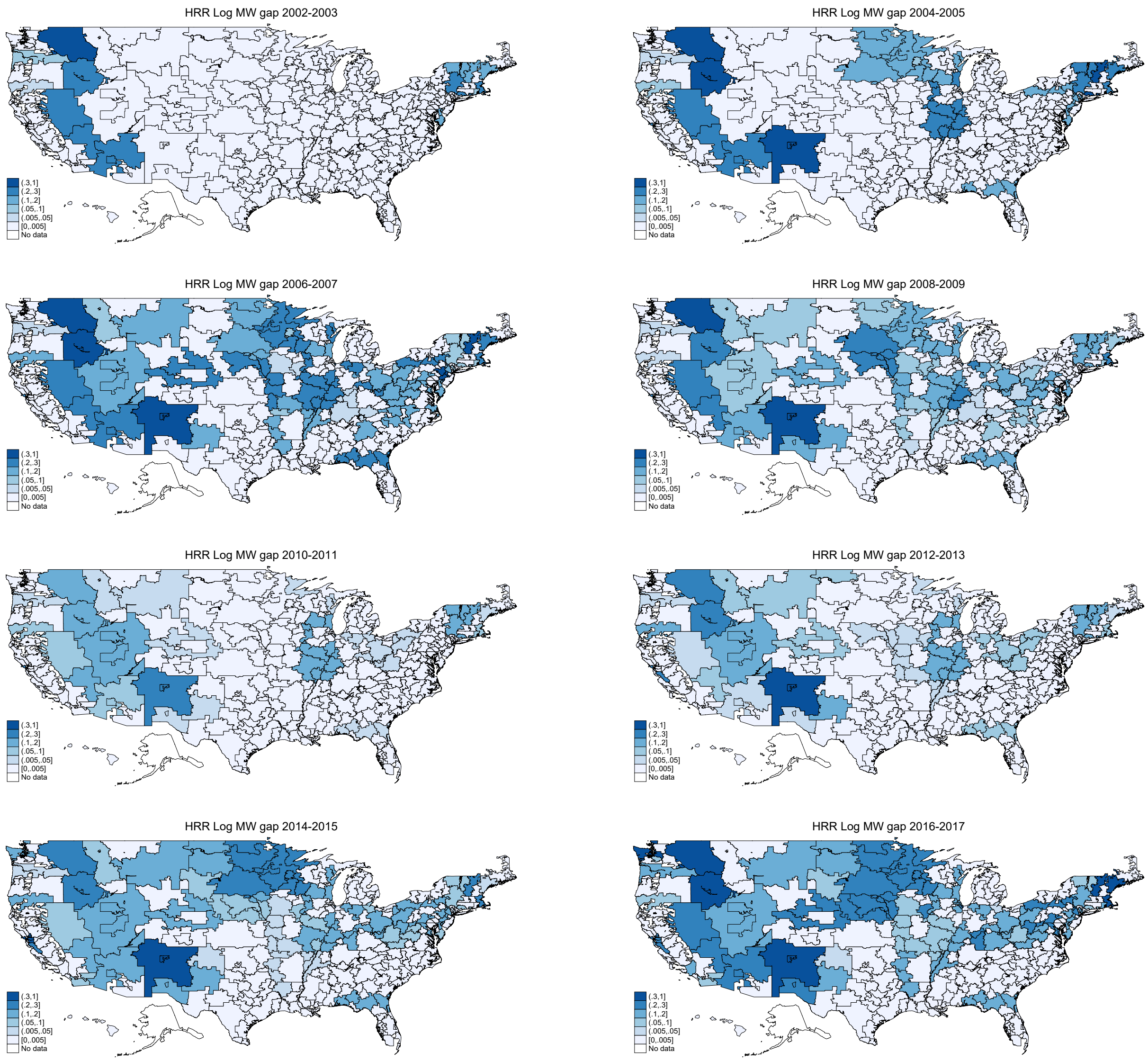
Notes: This figure plots the distribution of health inspection violations (left panel) and quality of care (right panel) occurring each inspection for the main analysis sample. Data are available for inspections occurring between 1998 and 2017. Sample is winsorized at the 99th percentile (26 total violations). Dashed lines denotes the sample median (5 for all violations, 3 for quality of care violations) and mean (6.6 for all violations, 3.6 for quality of care). See text for details.

Figure A.3: Age Adjusted and Raw Mortality Rate



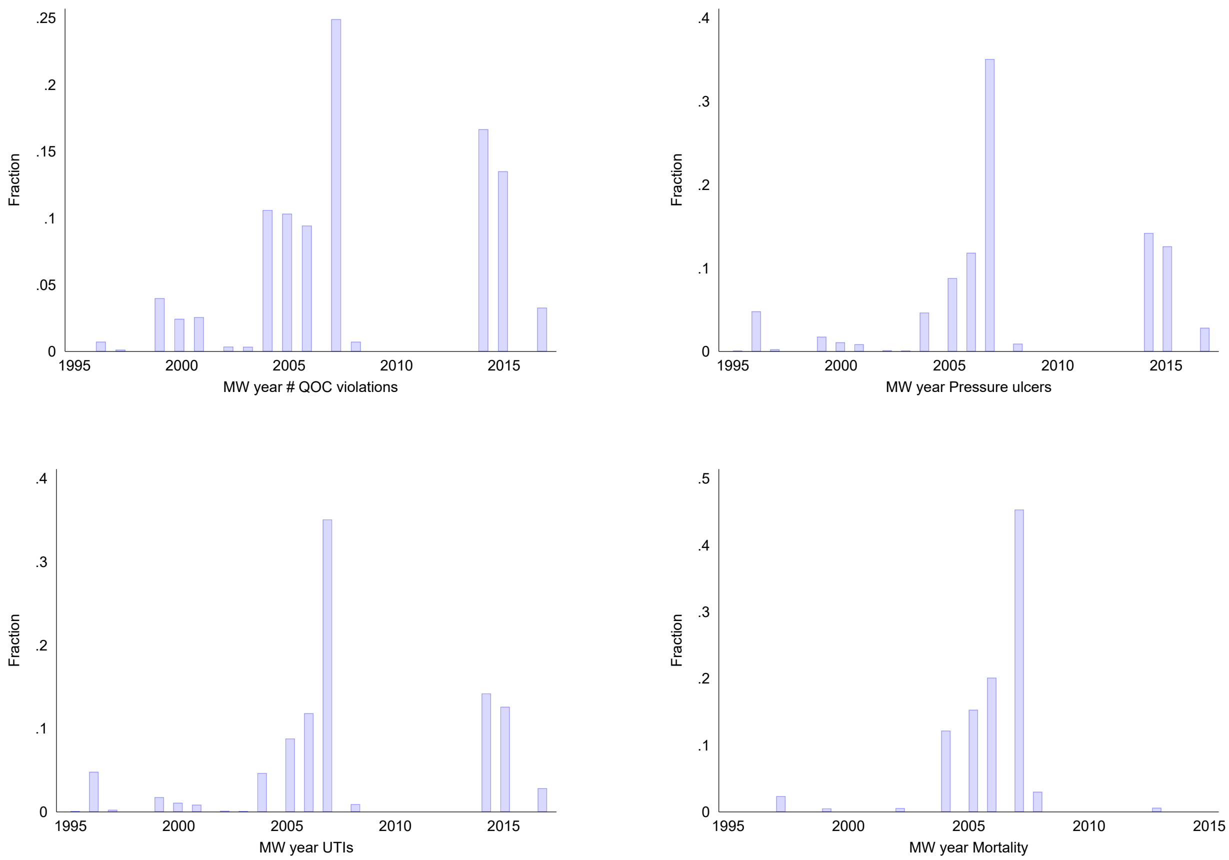
Notes: Figure shows the county-level age-adjusted (blue, open circle) and raw (red, closed circle) elderly mortality rate for 1990 through 2013. The age-adjusted series holds the age distribution of the elderly population fixed at year 2000 values. Age is topcoded at 85 years.

Figure A.4: Hospital Referral Region Log Minimum Wage Differential, by Year



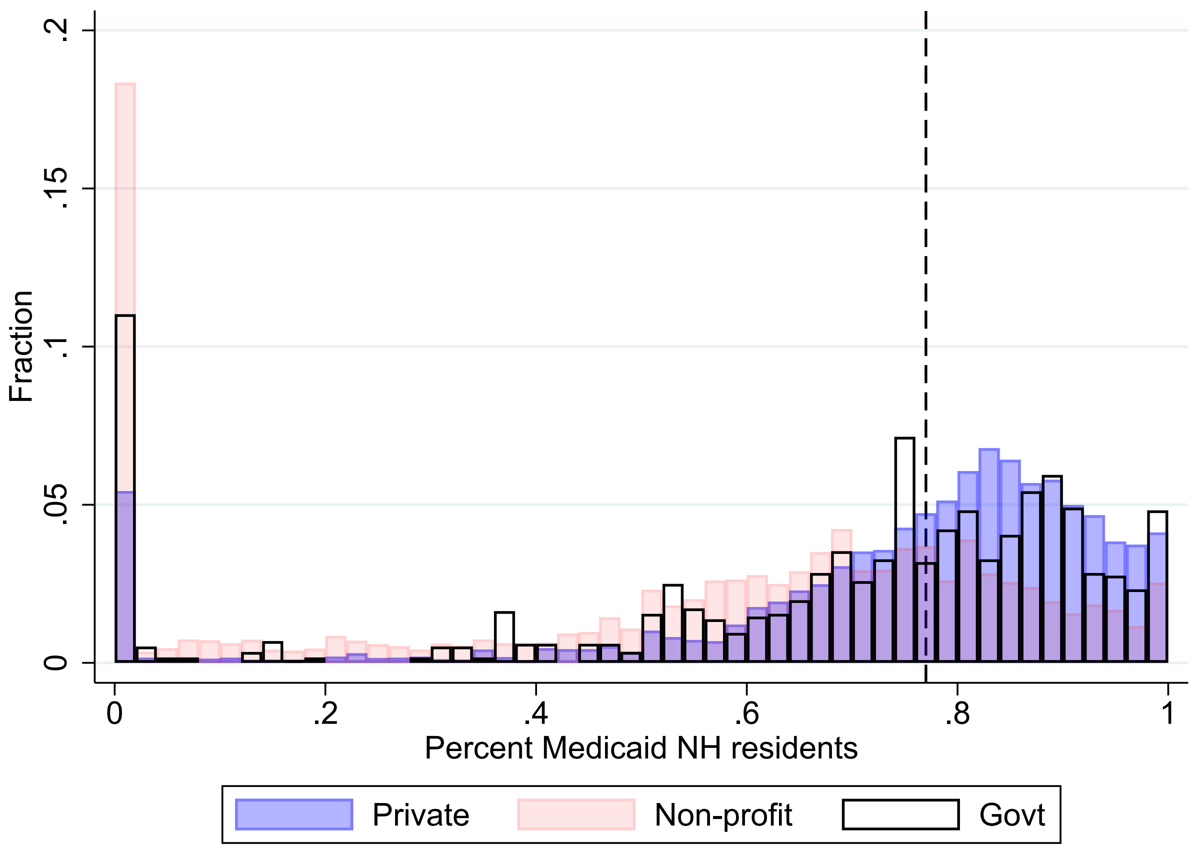
Notes: Figure shows the difference in (inflation-adjusted) log minimum wages between the jurisdiction with the highest statutory minimum and lowest statutory minimum within a Hospital Referral Region (HRR) for each two year period.

Figure A.5: Reform Years: Event Study



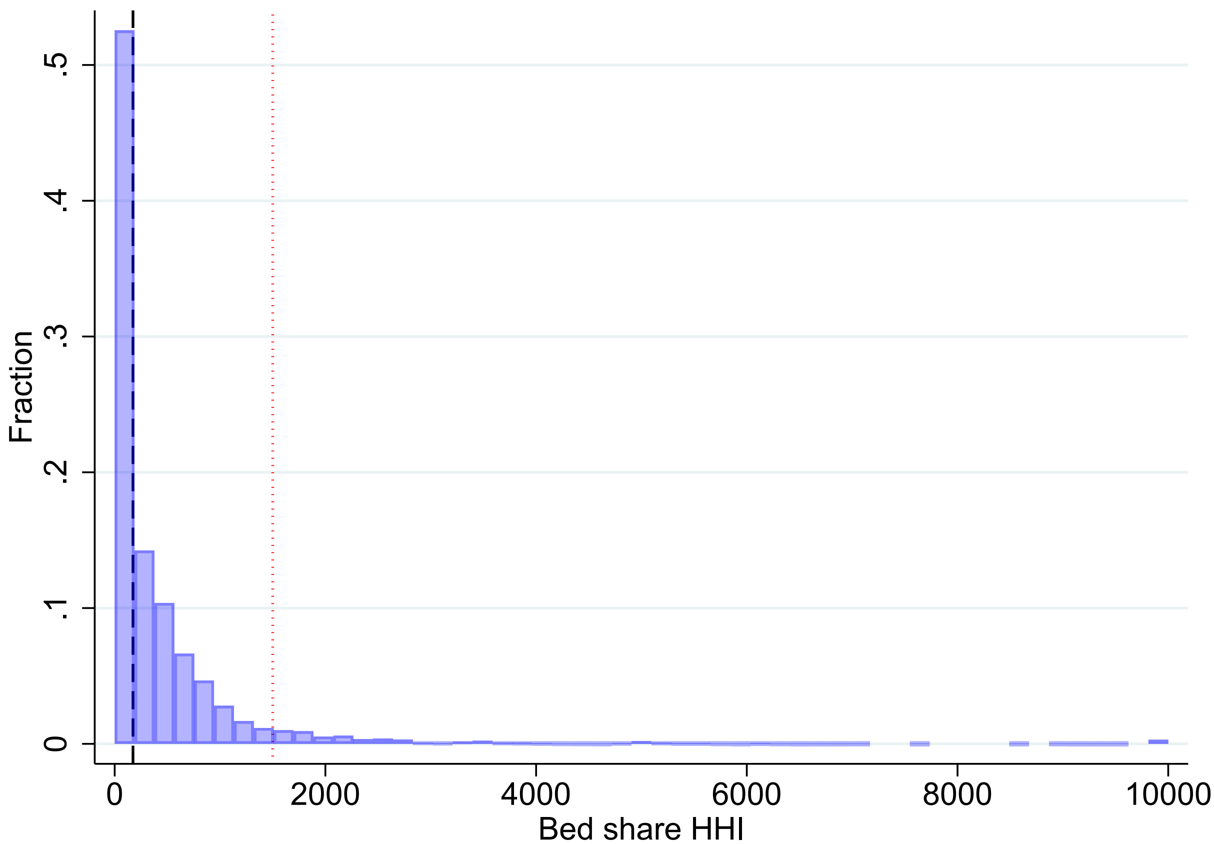
Notes: Figure shows reform years for event studies included in Figure 1.7. Sample is limited to reforms that changed the within-county-pair log gap by at least 5 log points and for which there were no changes greater than 0.5 log points in the preceeding six quarters (panels (a) through (c)) or four years (panel (d)). See text for details.

Figure A.6: Share of Residents Covered by Medicaid, by Ownership



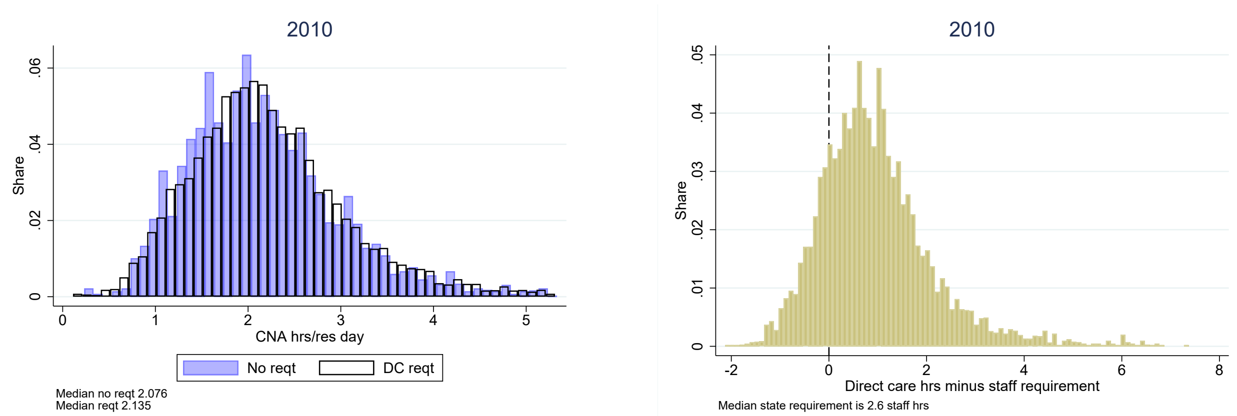
Notes:This figure plots the distribution of maximum Medicaid share for the 2000-2016 period among facilities that straddle a minimum wage discontinuity. The vertical dashed line is the sample median (77 percent).

Figure A.7: Nursing Home Market Concentration



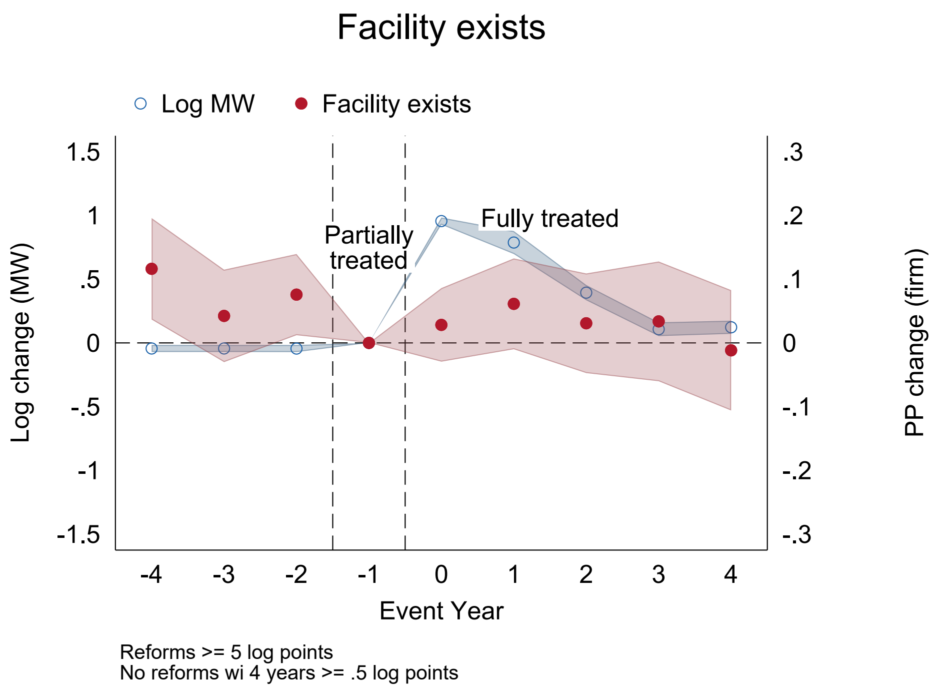
Notes: This figure plots the bed share HHI distribution for the 2000-2016 period among facilities that straddle a minimum wage discontinuity. The HHI ranges from 0 (perfect competition) to 10,000 (monopoly). The black vertical dashed line is the median over this period (278); the red dotted line denotes the threshold for a "highly competitive" market (1,500).

Figure A.8: Nursing Assistant Hours Per Resident Day, by Direct Care Staffing Requirement



Notes: Figure on left plots the distribution of nursing assistant hours per resident per day by whether a facility is located in a state requiring a minimum number of NA hours or employees. Right figure shows the difference in reported direct care hours and the state's minimum requirement for states with a staffing floor defined in staff hours per resident day as of 2010 (25). All calculations based on 2010 staffing requirements summarized in (Harrington, 2010).

Figure A.9: Firm Churn: Entry and Exits



Notes: Figure shows event studies from Equation 1.5. Blue line indicates the change in the minimum wage; red line shows the change in the likelihood (percentage points) a firm operates in a given year. Sample is limited to reforms that changed the within-county-pair log gap by at least 5 log points and for which there were no changes greater than 0.5 log points in the preceeding four years. Specifications includes county-pair-year, county, and reform year fixed effects. Shaded areas indicate 95 percent confidence intervals with robust standard errors clustered at the county level.

A.2 Worker effort and minimum wages

Section 1.3 provided intuition for how worker effort responds to higher wages and therefore may affect service quality. This appendix provides a slightly more formal discussion.

Nursing homes employ two types of nursing staff $i \in \{p = low - skilled, r = high - skilled\}$. Low-skilled labor, with subscript p, can be conceptualized as nursing assistants; high-skilled labor, r, are primarily RNs and physicians.

Generalizing the baseline Shapiro and Stiglitz (1984) model, individuals derive current utility from consumption and effort, $U_i(w, e) = u(w_i) - b_i(e_i)$, increasing and concave in after-tax wages w_i and decreasing and convex in effort e_i. The worker's effort function $b_i(.)$ is function that varies across individuals and potentially over time. That is, the cost of effort may depend on innate characteristics, such as a worker's comparative advantage, but also on factors that may evolve over time, such as her tenure with a particular employer or experience in the industry. Effort is not perfectly observable to firms; instead, employers observe a noisy signal of effort that is correct in expectation: $\widehat{e_i} = e_i + \mu_i$ where $\mathbb{E}(\mu_i) = 0$.

Each period, employed workers become unemployed with exogeneous probability q and are fired due to inadequate performance with probability $D(\widehat{e_i})$, a weakly decreasing function of observed effort $D^{'}(\widehat{e_i}) \leq 0$.[1] Unemployed workers find employment with exogeneous probability s. If unemployed, a worker receives $\overline{w}$ as income from unemployment insurance or income assistance programs.

Flow utility for individuals with discount rate r is given by:

$$V^i = \begin{cases} V^{w_i} = \frac{(1+r)(u(w_i) - b(e_i)) + (q + D(\widehat{e_i}))V_u}{r + q + D(\widehat{e_i})} & \text{if employed} \\ V^u = \frac{(1+r)u(\overline{w}) + sV^{w_i}}{r+s} & \text{if unemployed} \end{cases} \tag{A.1}$$

Taking wages w_i as given, workers choose an effort level to equate the expected present value of utility while unemployed to that while employed.

$$V^{w_i} = V^u \tag{A.2}$$

$$b_i(e_i) = u(w_i) - \frac{r}{1+r}V^{w_i} \tag{A.3}$$

$$e_i = b_i^{-1}\left((u(w_i) - u(\overline{w}))\,\frac{D(\widehat{e_i})(1-q)}{D(\widehat{e_i})(1-q) + (r+s+q)}\right) \tag{A.4}$$

Effort is higher the greater the wage: $\frac{de}{dw} > 0, and \frac{d^2e}{dwdw} < 0$ by the concavity of $u(w)$. Intuitively, higher wages increase the value of employment relative to non-employment, prompting workers to exert greater effort in order to reduce the likelihood of non-employment.

[1] This general framework is consistent with settings where worker effort is imperfectly observable and firms face monitoring costs increasing in the size of the workforce (Rebitzer and Taylor, 1995).

A.3 Nursing home wages, care, and social welfare

Section 1.3 examined the effects of higher wages on firm, employee, and consumer well-being in a stylized framework with three actors. This appendix providers a fuller analysis of the welfare implications of higher minimum wages from a social welfare perspective.

Building on the discussion in Section 1.3, workers are of two subtypes $i \in \{l = low-skilled, r = high-skilled\}$ $(w^l < w^r)$. These workers are employed by firm owners f, and provide care to two types of nursing home customers: private payors p and those covered by government programs g. g is modeled as a weighted average of Medicaid or Medicare. Each subpopulation's share of the overall population is given by θ^i, with welfare weights ζ^i, and $\overline{\zeta} = 1$. For ease of notation, define each subpopulation's welfare-weighted share of the total population as $\rho^i = \theta^i \zeta^i$.

Set-up

Workers As described in Section 1.3 and Appendix A.2, workers who are employed derive utility from consumption w^i and disutility for effort e^i, while those without work consume out of unemployment insurance or means-tested benefits $\overline{w}$ and do not incur effort.

Contemporaneous individual utility is therefore:

$$U^i = \begin{cases} u(w^i) - b_i(e^i) & \text{if employed} \\ u(\overline{w}) & \text{if unemployed} \end{cases} \tag{A.5}$$

The fraction of working-age individuals in each group with employment is L^i, and those without work $(1 - L^i)$.

Firm owners: Firm owners receive utility in the form of profits, where $U^f = \pi$ and $\pi = PX(Q, P, I, Z) - wL - rK$.

Nursing home residents: Potential nursing home consumers are elderly individuals who derive utility from health care quality Q obtained by accessing nursing home services X as in Section 1.3, and bequests m left to decedents: $V^i(Q, m) = \nu(Q) + z(m)$, where $\nu', z' > 0$ and $\nu'', z'' < 0$. The amount of nursing home services and bequests is determined by the wealth constraint $PX(Q, P, I, Z) + m \leq W$.

Government payors have wealth $W^g = 0$ and insurance $I^g = P^g$, and therefore have no out-of-pocket costs. Since $P^g = 0$ and $\frac{\partial V}{\partial Q} > 0$, these clients have perfectly inelastic demand for nursing home care. In particular, nursing homes are able to fill any beds not occupied by private payors with Medicaid recipients.

Private payors have accumulated wealth $W^p > 0$, incomplete insurance coverage $(I^p < P^p)$, and demand for nursing services increasing in quality and decreasing in price $(\frac{\partial X}{\partial Q} > 0, \frac{\partial X}{\partial P} < 0)$.

As in Gertler (1989), the number of nursing home beds is less than the elderly population $\theta^g + \theta^p < X$, the fraction of Medicaid recipients with access to care is $\Theta = \frac{X - X^p}{\theta^g + \theta^p}$.

Balanced budget requirement: Policymakers must finance Medicaid recipients' nursing home stay out of tax revenue. Government per-diem rates are given by $P_g < P$, with total cost $P_g \theta_g \Theta = T$.[2] Linear taxes at rate τ are levied on high-income workers' wages and firms' net profits to cover this care such that:

$$T = \Theta \theta_g P_g \tag{A.6}$$
$$T = \tau \left(w_r L_r \theta_r + \theta_f \pi \right) \tag{A.7}$$
$$\tau = \frac{\Theta \theta_g P_g}{\left(w_r L_r \theta_r + \theta_f \pi \right)} \tag{A.8}$$

Therefore, if quality improves, prices paid by private payors increase and firms serve additional private payors (see Section 1.3). Since the number of beds is fixed, fewer government beneficiaries obtain nursing services and government costs may fall. On the other hand, if Medicaid reimbursement rates are not fixed at P_g, but are a function of private rates ($P_g = \alpha P$, with $\alpha < 1$), the reduction in costs to taxpayers are partially offset by higher per-resident bed rates among those who continue to receive care. Therefore, even if firm profits and higher-income individuals are not directly affected changes in labor costs, they may be indirectly affected through changes in the financing requirements.

No minimum wages:

In the absence of minimum wages at time $t = 0$, social welfare is:

$$SW_0 = \rho^l \left(L_0^p (u(w_0^p) - b(e_0^p)) + (1 - L_0^p (u(\overline{w})) \right) \tag{A.9}$$
$$+ \rho^r \left(L_0^r (u(w_0^r) - b(e_0^r)) + (1 - L_0^r (u(\overline{w})) + \rho^f \pi_0 \right) \tag{A.10}$$
$$+ \rho^g \Theta_0 \nu(Q) + \rho^p \left(\nu(Q) + z(m) \right) \tag{A.11}$$

Minimum wages:

The introduction of a binding minimum wage at time $t = 1$ has several effects on the nursing home market (Section 1.3). First, low-skilled worker wages increase. Second, employment composition may change. Third, given higher costs of production for a given quality level, firm profits may fall, prices may increase, and service will change. The net effect is then:

[2] Appendix Table A.1 shows Medicaid reimbursement rates tend to be lower than average private rates.

$$\Delta SW = SW_1 - SW_0 \tag{A.12}$$

$$= \rho^l \left(\underbrace{L_1^l(\Delta(u(w^l) - b(e^l)))}_{\text{Employed low-skilled workers' earnings}} + \underbrace{(L_0^l - L_1^l)\left(u(\overline{w}) - (u(w_0^l) - b(e_0^l))\right)}_{\text{Change in low-skilled employt}} \right) \tag{A.13}$$

$$+ \rho^r \left(\underbrace{L_1^r(\Delta(u(w^r) - b(e^r)))}_{\text{Employed high-skilled workers' earnings}} + \underbrace{(L_0^r - L_1^r)\left(u(\overline{w}) - (u(w_0^r) - b(e_0^r))\right)}_{\text{Change in high-skilled employt}} \right) \tag{A.14}$$

$$+ \rho^f \Delta\pi + \underbrace{\rho^p \left(\Delta(\nu(Q) + z(m))\right)}_{\text{Private payor health net of price}} + \rho^g \left(\Theta_1 \underbrace{\Delta(\nu(Q))}_{\text{Govt health}} + \underbrace{\Delta\Theta\nu(Q_0}_{\text{Access change}}\ ) \right) \tag{A.15}$$

Without accounting for quality, the ν terms drop out. While ΔQ could theoretically be positive or negative, this paper documents that minimum wages improve quality in the nursing home sector. Therefore, accounting for quality improvements increases the desirability of minimum wages from a social welfare perspective.

A broader question, however, is whether increases in the minimum wage are socially desirable. This will depend on normative welfare weights attached to each population, aggregate employment and income changes, the strength of the relationship between higher wages and service quality, and the price elasticity of private demand for nursing home services. For policymakers with redistributional preferences, where $\zeta^f < \zeta^r < \zeta^p$ and $\zeta^w < \zeta^g$, minimum wages are most likely to be welfare-improving when the private demand with respect to quality, $\frac{\partial X_p}{\partial Q}$ is relatively inelastic, but higher wages are an effective tool for inducing improved performance ($\frac{\partial Q}{\partial W_p} > 0$ is large). From a political economy lens, policymakers will favor minimum wages if the median voter benefits. Given relatively high voter turnout among elderly and higher-income individuals, wage increases are most likely to be legislated when higher wages yield quality improvements (and particularly when turnout is increasing in both income and age).

Applying the empirical results from Section 1.5, this paper finds:

$$\Delta(u(w^l) - b(e^l)) \geq 0 \tag{A.16}$$
$$(L_0^l - L_1^l)\left(u(\overline{w}) - (u(w_0^l) - b(e_0^l))\right) \approx 0 \tag{A.17}$$
$$(L_0^r - L_1^r)\left(u(\overline{w}) - (u(w_0^r) - b(e_0^r))\right), \Delta(u(w^r) - b(e^r)) \approx 0 \tag{A.18}$$
$$\Delta\pi \approx 0 \tag{A.19}$$
$$(\Delta(\nu(Q) + z(m))) \geq 0 \tag{A.20}$$
$$\Delta(\nu(Q)) > 0 \tag{A.21}$$
$$\Delta\Theta < 0 \tag{A.22}$$

Therefore, modest increases in the minimum wage are welfare-improving if and only if:

$$\rho^l \left(L_1^l(\Delta(u(w^l) - b(e^l)))\right) + \rho^p \left(\Delta(\nu(Q) + z(m))\right) + \rho^g \Theta_1 \nu(Q_1) > \rho^g \Theta_0 \nu(Q_0) \tag{A.23}$$

Appendix B

Universal Access to Free School Meals and Student Achievement

B.1 Tables and Figures

Table B.1: Effect of CEP on Meal Consumption: Parametric Event Study

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Breakfast				Lunch			
	All	All	Exposed	Exposed	All	All	Exposed	Exposed
Event year	5.982	4.981	2.601**	1.979	4.808	3.703	0.648	-4.487
	(3.460)	(3.419)	(0.970)	(5.410)	(2.966)	(2.813)	(1.229)	(2.635)
Post	9.983***	9.767***	13.330***	12.379***	10.245***	10.329***	10.433***	10.101***
	(1.719)	(1.608)	(1.838)	(1.749)	(0.851)	(0.828)	(1.036)	(1.100)
Event year X post	0.078	0.206	1.839	0.769	1.701	1.917	3.152***	3.231***
	(0.996)	(1.056)	(1.544)	(1.510)	(0.978)	(0.984)	(0.780)	(0.886)
StateXyear trends	X	X	X	X	X	X	X	X
Baseline var trends		X		X		X		X
Observations	14248	14248	6003	6003	14269	14269	6013	6013

Notes: Table presents unweighted results from meal count data collected from state Department of Educations for six of the eleven states that adopted CEP before 2015: Georgia, Illinois, Kentucky, New York, Maryland, and West Virginia. Data availability varies by state, but spans 2009-2016. All specifications include controls for student demographics, the fraction of charter schools in a district, child poverty and unemployment rates, and measures of racial/ethnic segregation, as well as year and school fixed effects. Even-numbered columns also include state-specific linear trends and trends in baseline variables. Robust standard errors clustered by district. Columns (1-2) and (5-6) ("all") include all observations that adopted CEP between 2012 and 2017; columns (3-4) and (7-8) ("exposed") restrict the sample to observations in districts with a baseline FRP eligibility rate below 57.9 percent (the median among CEP-adopting districts). $\beta_{ey} = \beta_{ey*post}$ presents p-value from a hypothesis test that the pre-CEP linear trends equals the trend after CEP adoption. Robust standard errors clustered by district. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table B.2: Effect of CEP on Meal Consumption: Linear Trends by State and Baseline Variables

	(1)	(2)	(3)	(4)	(5)	(6)
	School per-student breakfast		School per-student lunch		Log per student nutrit asst	
	All	Exposed	All	Exposed	All	Exposed
CEP	12.102***	12.520***	12.371***	12.129***	0.074***	0.082***
	(2.167)	(2.754)	(1.259)	(1.415)	(0.009)	(0.012)
Observations	18762	12077	20030	13193	128145	64105
Basline DV mean	52.57	49.16	111.9	104.3	0.400	0.327
Pct change	0.230	0.255	0.111	0.116		
StateXyear trends	X	X	X	X	X	X
Baseline trends	X	X	X	X	X	X
Level	School	School	School	School	District	District

Notes: Table presents unweighted results from estimating Equation 2.1 at the school level (columns (1) through (4)) with meal count data collected from state Department of Educations for six of the eleven states that adopted CEP before 2015: Georgia, Illinois, Kentucky, New York, Maryland, and West Virginia. Data availability varies by state, but spans 2009-2016. Columns 5 and 6 presents federal nutritional assistance dollars, reported in the Annual Survey of School System Finances. All specifications include controls for student demographics, the fraction of charter schools in a district, child poverty and unemployment rates, and measures of racial/ethnic segregation, as well as year and school fixed effects. Robust standard errors clustered by district. Odd-numbered columns ("all") include all observations that adopted CEP between 2012 and 2017; even-numbered columns ("exposed") restrict the sample to observations in districts with a baseline FRP eligibility rate below 57.9 percent (the median among CEP-adopting districts). Robust standard errors clustered by district. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table B.3: Federal Nutritional Assistance ($1,000s) and Overall Student Performance

	(1) Math	(2) Math	(3) Reading	(4) Reading
Per-student fed nutr. asst	0.163	0.512*	-0.142	0.235
	(0.212)	(0.296)	(0.142)	(0.206)
Observations	59465	31423	62174	32968
Sample	All	Exposed	All	Exposed
Baseline DV mean (level)	-0.247	-0.118	-0.232	-0.095
Change in nutritional asst	0.100	0.094	0.099	0.093
F stat 1st stage	184.856	106.867	156.994	105.841

Notes: Table presents 2SLS regression results where the change in per-student federal nutritional assistance is instrumented by CEP participation. "Exposed" districts are district-grade observations with a baseline FRP eligibility share below 57.9 percent (the baseline median among CEP districts) in which any school serving grade g participated in CEP by 2017; treatment districts are districts in which at least one school adopts CEP by 2015. "All" districts include all district-grade observations that participated in CEP at any point by 2017. All specifications include district, grade, and year fixed effects, as well as student racial/ethnic composition and segregation, percent of students attending a charter school, child poverty rates and county unemployment rates. All specifications are weighted least squares, with weights equal to the squared inverse of the standard error of the district-grade performance metric. Robust standard errors clustered by district. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table B.4: Predicted Performance from Changes in Racial/Ethnic Composition

	(1)	(2)	(3)	(4)
	$\widehat{Overall}$	$\widehat{Black}$	$\widehat{Hispanic}$	$\widehat{White}$
	Panel A: Math performance			
CEP	-0.001	-0.002**	0.001	0.001
	(0.001)	(0.001)	(0.003)	(0.001)
Observations	32694	11658	12698	29325
Baseline FRP	0.454	0.457	0.438	0.458
Baseline DV mean	0.077	-0.418	-0.225	0.214
	Panel B: Reading performance			
CEP	-0.000	-0.002**	-0.001	0.002
	(0.001)	(0.001)	(0.002)	(0.001)
Observations	34344	12185	13256	30581
Baseline FRP	0.453	0.457	0.436	0.458
Baseline DV mean	0.075	-0.421	-0.226	0.213
Area and district controls	X	X	X	X
Sample	Exposed	Exposed	Exposed	Exposed

Notes: Table presents weighted least squares regression results from the specification in Equation 2.1 for district-grade observations with a baseline FRP eligibility share below 57.9 percent (the baseline median among CEP districts) in which any school serving grade g participated in CEP by 2017; treatment districts are districts in which at least one school adopts CEP by 2015. Race/ethnic proficiency scores available for cells with at least 20 students. All specifications include district, grade, and year fixed effects, student-teacher ratios, percent of students attending a charter school, child poverty rates and county unemployment rates. Dependent variable is defined as predicted values from a regression interacting each grade with the share of students of each racial/ethnic group in a district and CEP schools within a district, as well as the dissimilarity index for each racial/ethnic group. Robust standard errors clustered by district. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table B.5: Effect of CEP on Math Performance: High-Exposure Districts Sample, Alternative Specifications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: All							
CEP	0.015	0.011	0.008	0.013	0.002	0.016*	0.018**
	(0.009)	(0.009)	(0.007)	(0.009)	(0.010)	(0.008)	(0.008)
Observations	32694	32607	26301	32694	32694	32694	32645
Baseline FRP	0.454	0.454	0.455	0.454	0.454	0.454	0.454
Baseline DV mean	-0.121	-0.121	-0.116	-0.121	-0.121	-0.121	-0.121
Panel B: Black							
CEP	0.024	0.011	0.010	0.022	-0.002	0.001	0.006
	(0.018)	(0.016)	(0.014)	(0.020)	(0.021)	(0.014)	(0.014)
Observations	11658	11658	8996	11658	11658	11658	11658
Baseline FRP	0.457	0.457	0.459	0.457	0.457	0.457	0.457
Baseline DV mean	-0.502	-0.502	-0.500	-0.502	-0.502	-0.502	-0.502
Treatment defn	Binary	Binary	Binary	Binary	Pct	Binary	Binary
Resource variables	X						
State X year trends		X	X				
Baseline trends		X	X				
Lagged performance			X				
State X year FE				X			
Weights	WLS	WLS	WLS	WLS	WLS	District	Log enroll

Table B.5: (continued)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
				Panel C: Hispanic			
CEP	0.031**	0.029**	0.029***	0.027*	0.026	0.025*	0.029**
	(0.016)	(0.014)	(0.011)	(0.016)	(0.018)	(0.013)	(0.013)
Observations	12698	12679	9582	12698	12698	12698	12679
Baseline FRP	0.438	0.438	0.437	0.438	0.438	0.438	0.438
Baseline DV mean	-0.315	-0.315	-0.305	-0.315	-0.315	-0.315	-0.315
				Panel D: White			
CEP	0.017*	0.010	0.006	0.016*	-0.001	0.019**	0.021**
	(0.010)	(0.009)	(0.008)	(0.010)	(0.011)	(0.009)	(0.009)
Observations	29325	29272	23324	29325	29325	29325	29293
Baseline FRP	0.458	0.458	0.459	0.458	0.458	0.458	0.458
Baseline DV mean	0.011	0.011	0.015	0.011	0.011	0.011	0.011
Treatment defn	Binary	Binary	Binary	Binary	Pct	Binary	Binary
Resource variables	X						
State X year trends		X	X				
Baseline trends		X	X				
Lagged performance			X				
State X year FE				X			
Weights	WLS	WLS	WLS	WLS	WLS	District	Log enroll

Notes: Table presents regression results from the specification in Equation 2.1 for district-grade observations with a baseline FRP eligibility share below 57.9% in which any school serving grade g participated in CEP by 2017; treatment districts are districts in which at least one school adopts CEP by 2015. All specifications include district, grade, and year fixed effects, as well as student racial/ethnic composition and segregation, student-teacher ratios, percent of students attending a charter school, child poverty rates and county unemployment rates. "Resource variables" include per-pupil total and instructional expenditures; "baseline trends" includes linear trends for baseline values of all control variables. Columns (1-4) present WLS regressions with additional controls; column (5) presents unweighted results; column (6) weights each observation by the log number of students in each racial/ethnic group between 2009 and 2011. Robust standard errors clustered by district. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table B.6: Effect of CEP on Reading Performance: High-Exposure Districts Sample, Alternative Specifications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: All							
CEP	0.007	0.005	0.001	-0.001	-0.002	0.007	0.008
	(0.006)	(0.005)	(0.004)	(0.005)	(0.007)	(0.007)	(0.006)
Observations	34344	34250	28329	34344	34344	34344	34291
Baseline FRP	0.453	0.453	0.454	0.453	0.453	0.453	0.453
Baseline DV mean	-0.104	-0.104	-0.101	-0.104	-0.104	-0.104	-0.104
Panel B: Black							
CEP	0.014	0.008	0.001	0.006	-0.019	-0.007	-0.002
	(0.009)	(0.007)	(0.007)	(0.009)	(0.017)	(0.011)	(0.011)
Observations	12185	12185	9504	12185	12185	12185	12185
Baseline FRP	0.457	0.457	0.458	0.457	0.457	0.457	0.457
Baseline DV mean	-0.441	-0.441	-0.441	-0.441	-0.441	-0.441	-0.441
Treatment defn	Binary	Binary	Binary	Binary	Pct	Binary	Binary
Resource variables	X						
State X year trends		X	X				
Baseline trends		X	X				
Lagged performance			X				
State X year FE				X			
Weights	WLS	WLS	WLS	WLS	WLS	District	Log enroll

Table B.6: (continued)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
				Panel C: Hispanic			
CEP	0.016	0.018**	0.005	0.011	0.014	-0.000	0.006
	(0.010)	(0.009)	(0.009)	(0.010)	(0.015)	(0.011)	(0.011)
Observations	13256	13234	10110	13256	13256	13256	13236
Baseline FRP	0.436	0.436	0.434	0.436	0.436	0.436	0.436
Baseline DV mean	-0.391	-0.391	-0.385	-0.391	-0.391	-0.391	-0.391
				Panel D: White			
CEP	0.007	0.004	-0.000	-0.001	-0.004	0.010	0.011
	(0.007)	(0.006)	(0.005)	(0.006)	(0.008)	(0.007)	(0.007)
Observations	30581	30530	24789	30581	30581	30581	30550
Baseline FRP	0.458	0.458	0.459	0.458	0.458	0.458	0.458
Baseline DV mean	0.051	0.051	0.054	0.051	0.051	0.051	0.051
Treatment defn	Binary	Binary	Binary	Binary	Pct	Binary	Binary
Resource variables	X						
State X year trends		X	X				
Baseline trends		X	X				
Lagged performance			X				
State X year FE				X			
Weights	WLS	WLS	WLS	WLS	WLS	District	Log enroll

Notes: Table presents regression results from the specification in Equation 2.1 for district-grade observations with a baseline FRP eligibility share below 57.9% in which any school serving grade g participated in CEP by 2017; treatment districts are districts in which at least one school adopts CEP by 2015. All specifications include district, grade, and year fixed effects, as well as student racial/ethnic composition and segregation, student-teacher ratios, percent of students attending a charter school, child poverty rates and county unemployment rates. "Resource variables" include per-pupil total and instructional expenditures; "baseline trends" includes linear trends for baseline values of all control variables. Columns (1-4) present WLS regressions with additional controls; column (5) presents unweighted results; column (6) weights each observation by the log number of students in each racial/ethnic group between 2009 and 2011. Robust standard errors clustered by district. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table B.7: Effect of CEP on Math Performance: High-Exposure Districts Sample, Alternative Samples

	(1)	(2)	(3)
Panel A: All			
CEP	0.014	0.015	0.005
	(0.016)	(0.011)	(0.016)
Observations	14835	22162	12948
Baseline FRP	0.468	0.458	0.455
Baseline DV mean	-0.168	-0.098	-0.126
Panel B: Black			
CEP	-0.024	0.021	0.017
	(0.035)	(0.023)	(0.031)
Observations	2734	6636	5228
Baseline FRP	0.502	0.465	0.455
Baseline DV mean	-0.546	-0.487	-0.495
Sample	Full dist	Balanced	Adopt 1st yr

Table B.7: (continued)

	(1)	(2)	(3)
	Panel C: Hispanic		
CEP	0.007	0.031	0.010
	(0.037)	(0.019)	(0.031)
Observations	2956	6552	6146
Baseline FRP	0.453	0.433	0.438
Baseline DV mean	-0.365	-0.273	-0.289
	Panel D: White		
CEP	0.021	0.017	0.001
	(0.017)	(0.012)	(0.017)
Observations	12954	18949	11350
Baseline FRP	0.474	0.462	0.462
Baseline DV mean	-0.074	0.040	0.024
Sample	Full dist	Balanced	Adopt 1st yr

Notes: Table presents weighted least squares regression results from the specification in Equation 2.1 for district-grade observations with a baseline FRP eligibility share below 57.9 percent (the baseline median among CEP districts) in which any school serving grade g participated in CEP by 2017; treatment districts are districts in which at least one school adopts CEP by 2015. Race/ethnic proficiency scores available for cells with at least 20 students. ‘All specifications include district, grade, and year fixed effects, as well as student racial/ethnic composition and segregation, student-teacher ratios, percent of students attending a charter school, child poverty rates and county unemployment rates. Column (1) restricts to district-grade observations where every school serving grade g participates in CEP upon CEP adoption. Column (2) limits the sample to district-grade observations with a valid performance score each year. Column (3) limits the sample to districts that participated in CEP the first year their state became eligible. Robust standard errors clustered by district. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table B.8: Effect of CEP on Reading Performance: High-Exposure Districts Sample, Alternative Samples

	(1)	(2)	(3)
Panel A: All			
CEP	0.021*	-0.000	-0.021***
	(0.013)	(0.006)	(0.008)
Observations	15373	26999	13720
Baseline FRP	0.467	0.457	0.454
Baseline DV mean	-0.144	-0.096	-0.142
Panel B: Black			
CEP	-0.007	0.005	0.003
	(0.027)	(0.010)	(0.012)
Observations	2782	7504	5397
Baseline FRP	0.501	0.462	0.455
Baseline DV mean	-0.472	-0.445	-0.462
Sample	Full dist participation	Balanced panel	Adopt 1st yr eligibility

Table B.8: (continued)

	(1)	(2)	(3)
	Panel C: Hispanic		
CEP	0.004	0.006	0.002
	(0.032)	(0.010)	(0.011)
Observations	3037	7028	6432
Baseline FRP	0.448	0.427	0.435
Baseline DV mean	-0.477	-0.388	-0.413
	Panel D: White		
CEP	0.024*	0.001	-0.028***
	(0.014)	(0.007)	(0.009)
Observations	13295	22302	11861
Baseline FRP	0.474	0.462	0.462
Baseline DV mean	-0.025	0.053	0.046
Sample	Full dist participation	Balanced panel	Adopt 1st yr eligibility

Notes: Table presents weighted least squares regression results from the specification in Equation 2.1 for district-grade observations with a baseline FRP eligibility share below 57.9 percent (the baseline median among CEP districts) in which any school serving grade g participated in CEP by 2017; treatment districts are districts in which at least one school adopts CEP by 2015. Race/ethnic proficiency scores available for cells with at least 20 students. ‘All specifications include district, grade, and year fixed effects, as well as student racial/ethnic composition and segregation, student-teacher ratios, percent of students attending a charter school, child poverty rates and county unemployment rates. Column (1) restricts to district-grade observations where every school serving grade g participates in CEP upon CEP adoption. Column (2) limits the sample to district-grade observations with a valid performance score each year. Column (3) limits the sample to districts that participated in CEP the first year their state became eligible. Robust standard errors clustered by district. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table B.9: Effects of CEP on Reading Performance, Exposure Distribution

	(1)	(2)	(3)	(4)	(5)
Baseline FRP eligible	$\leq$ 40%	$\leq$ 50%	$\leq$ 60%	$\leq$ 70%	$\leq$ 80%
	Panel A: Overall performance				
CEP	0.012	0.006	0.006	0.001	-0.003
	(0.013)	(0.007)	(0.006)	(0.006)	(0.005)
Observations	8054	21276	37599	51906	61616
Average baseline FRP	0.312	0.401	0.465	0.515	0.551
Baseline DV mean	-0.034	-0.065	-0.116	-0.175	-0.220
	Panel B: Black performance				
CEP	0.030	0.015	0.010	-0.003	-0.006
	(0.028)	(0.012)	(0.009)	(0.010)	(0.009)
Observations	2834	7631	13590	21013	27147
Average baseline FRP	0.325	0.407	0.471	0.534	0.582
Baseline DV mean	-0.383	-0.415	-0.450	-0.488	-0.510
	Panel C: Hispanic performance				
CEP	0.033	0.016	0.013	0.003	0.004
	(0.023)	(0.013)	(0.010)	(0.009)	(0.008)
Observations	3976	8605	14499	19961	23970
Average baseline FRP	0.294	0.380	0.449	0.503	0.543
Baseline DV mean	-0.358	-0.363	-0.394	-0.422	-0.445

Table B.9: (continued)

	(1)	(2)	(3)	(4)	(5)
Baseline FRP eligible	$\leq 40\%$	$\leq 50\%$	$\leq 60\%$	$\leq 70\%$	$\leq 80\%$
	Panel D: White performance				
CEP	0.011	0.004	0.008	0.004	0.002
	(0.014)	(0.008)	(0.006)	(0.006)	(0.005)
Observations	6703	19017	33385	44762	51077
Average baseline FRP	0.326	0.410	0.469	0.514	0.542
Baseline DV mean	0.148	0.079	0.046	0.023	0.006
Percentile baseline FRP distribution	11.700	31.000	54.800	75.500	89.600

Notes: Table presents results from Equation 2.1 for all district-grade observations in which any school serving grade g participated in CEP by 2017 based on the baseline (2009-2011) share of students FRP eligible under the traditional formula. $CEP = 1$ if any school serving grade g in district d participated in CEP by year t. Race/ethnic proficiency scores available for cells with at least 20 students. "Average baseline FRP" indicates average baseline eligibility rates. "Percentile baseline FRP distribution" displays the share of districts with baseline eligibility $\leq x\%$. All specifications include district, grade, and year fixed effects, as well as student racial/ethnic composition and segregation, student-teacher ratios, percent of students attending a charter school, child poverty rates and county unemployment rates. Robust standard errors clustered by district. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Figure B.1: Math Performance Event Study, Robustness, Exposed Districts

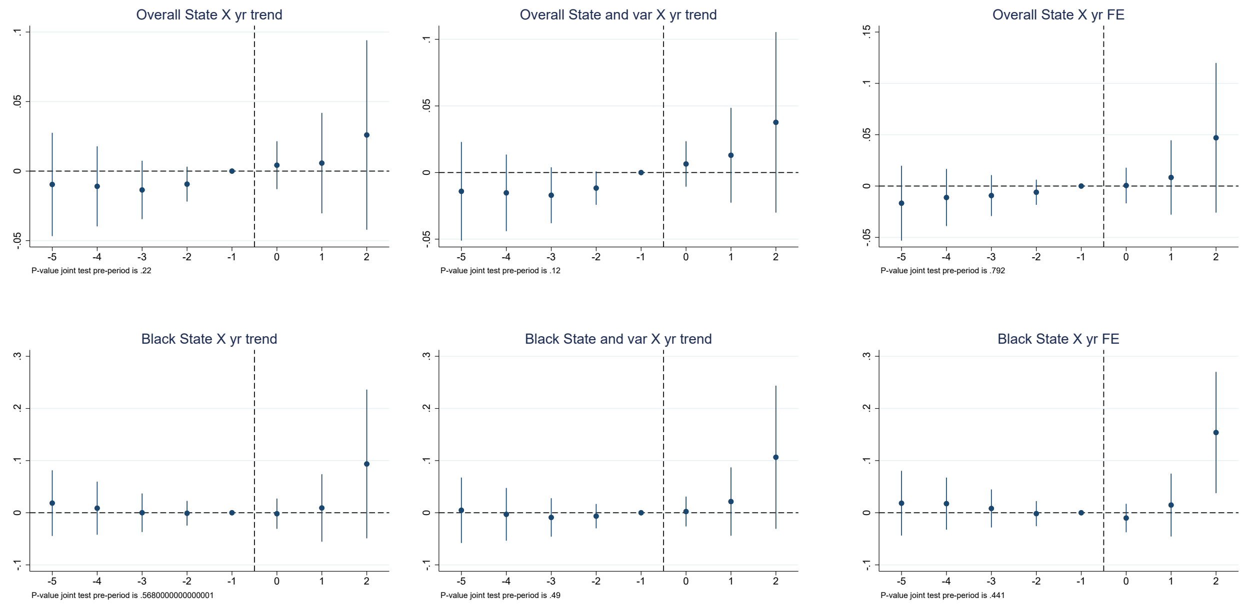
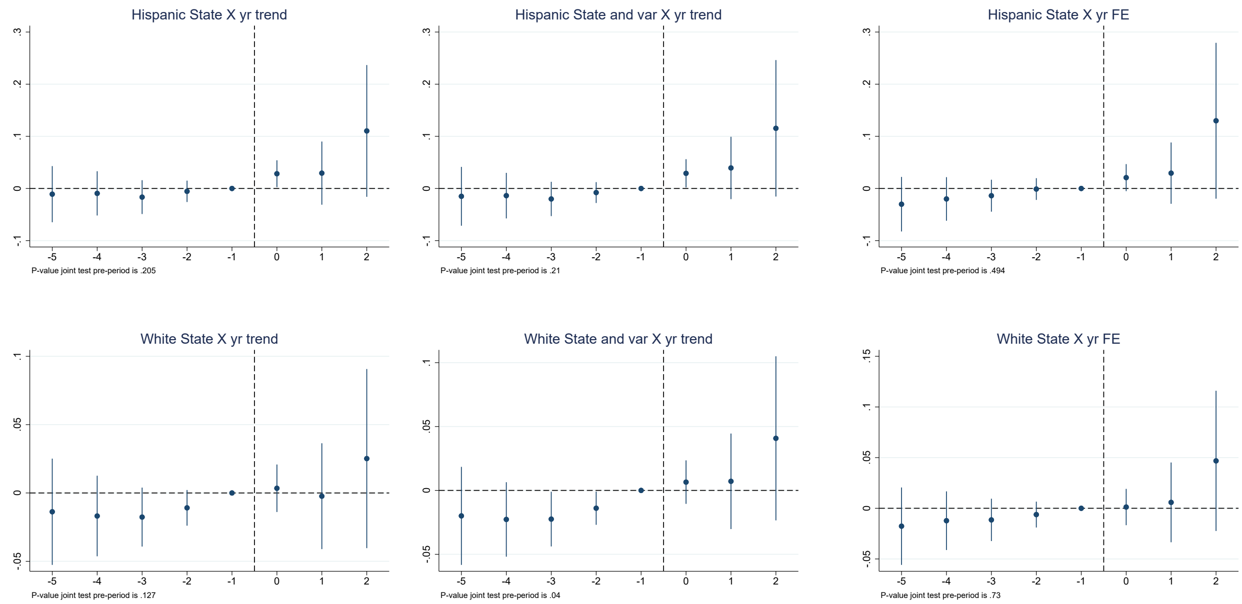
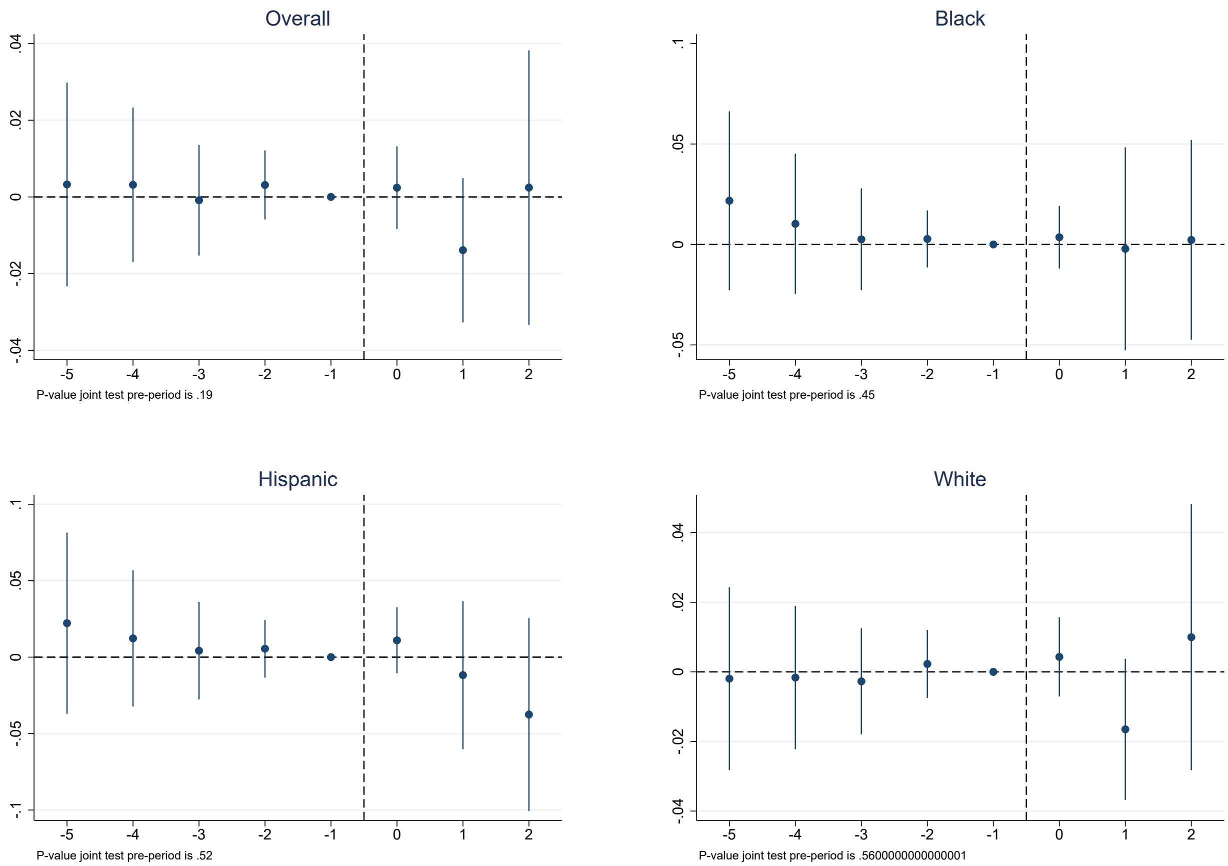


Figure B.2: (continued)



Notes: Figure presents results from event study framework in Equation 2.2. All specifications include controls for student demographics, the fraction of charter schools in a district, child poverty and unemployment rates, and measures of racial/ethnic segregation, year fixed effects, grade fixed effects, and district fixed effects. Left and center panels include state linear trends, center panel also includes linear trends in baseline covariates. Right panel includes state-by-year fixed effects. Bars denote 95 percent confidence intervals from robust standard errors clustered by district. Sample includes districts with a baseline FRP eligibility rate below 57.9 percent (the median among CEP-adopting districts). Notes below each panel present p-values from the joint test that pre-treatment coefficients equal to zero.

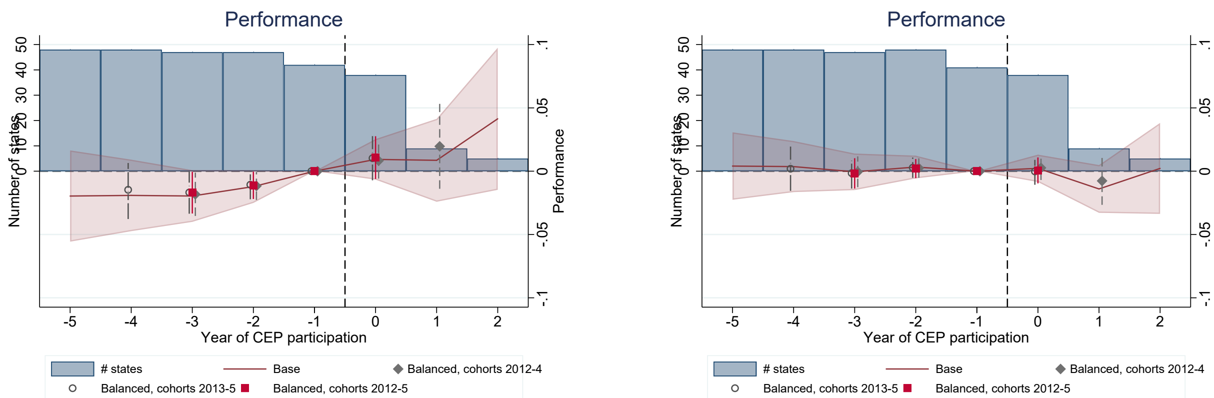
Figure B.3: Reading Performance Event Study, Exposed Districts



Notes: Figure presents results from the (district-level) event study framework in Equation 2.2. All specifications include controls for student demographics, the fraction of charter schools in a district, child poverty and unemployment rates, and measures of racial/ethnic segregation, year fixed effects, grade fixed effects, and district fixed effects. Bars denote 95 percent confidence intervals from robust standard errors clustered by district. Sample includes districts with a baseline FRP eligibility rate below 57.9 percent (the median among CEP-adopting districts). Notes below each panel present p-values from the joint test that pre-treatment coefficients equal to zero.

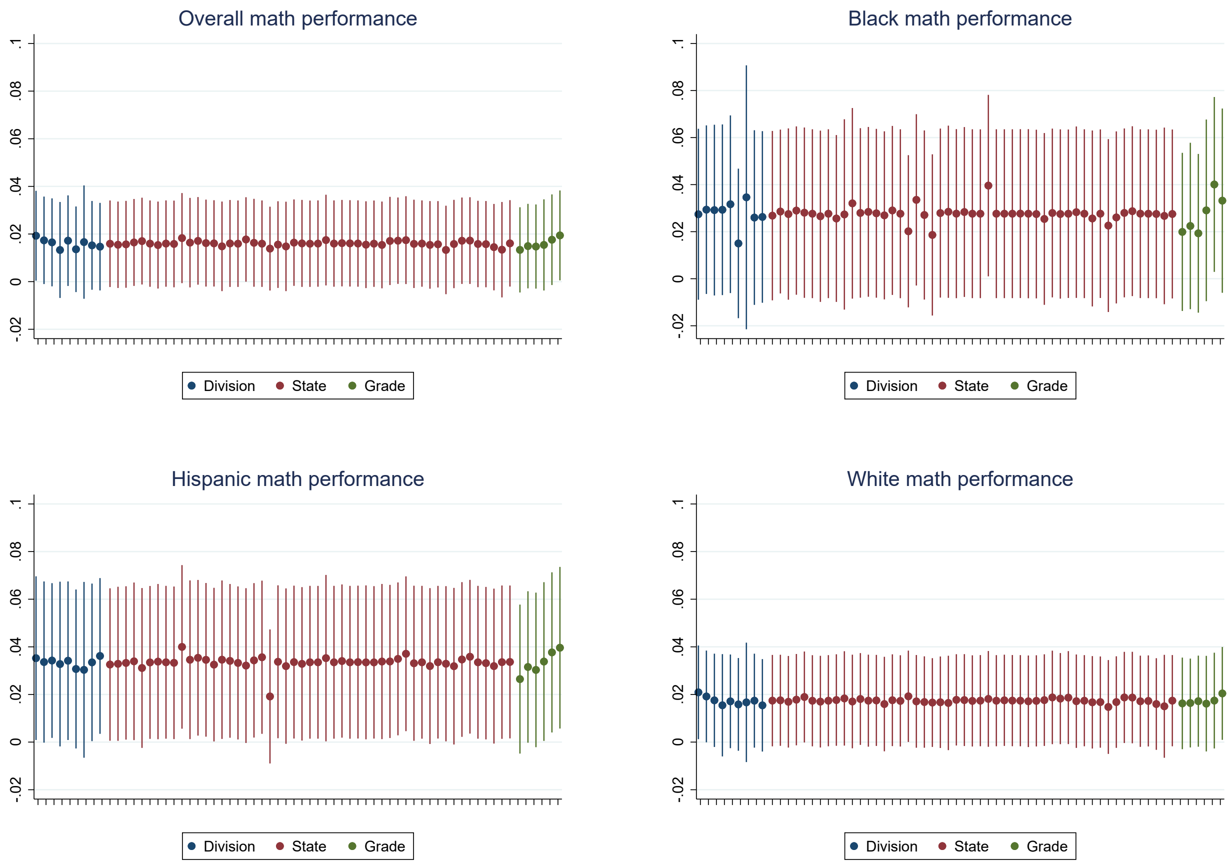
Figure B.4: Overall Performance: Balanced and Unbalanced Event Studies

(a) Math (b) Reading



Notes: Figure summarizes the number of states contributing to each event year in the unbalanced panel (blue bars), and presents results from event study framework in Equation 2.2, with event years defined as year relative to CEP implementation for both unbalanced (maroon line) and three balanced subpanels. The gray diamonds show the balanced panel among districts that first adopted CEP between 2012 and 2014; the open gray circles show the 2013-2015 cohorts; and the bright red squares show the balanced event study for districts that adopted within the 2012 through 2015 period. All specifications include controls for student demographics, the fraction of charter schools in a district, child poverty and unemployment rates, and measures of racial/ethnic segregation, year fixed effects, grade fixed effects, and district fixed effects. 95 percent confidence intervals from robust standard errors clustered by district. Sample includes districts with a baseline FRP eligibility rate below 57.9 percent (the median among CEP-adopting districts).

Figure B.5: Math Performance: Drop Division, State, Grade



Notes: Figure plots coefficients and confidence intervals from the specifications in Table 2.5 for district-grade observations with a baseline FRP eligibility share below 57.9 percent (the baseline median among CEP districts) in which any school serving grade *g* participated in CEP by 2017, but dropping a single Census Division (blue), state (red), or grade (green). All omitted areas and grades are in ascending order (e.g.: the far-left point is Census Division 1, Alabama, or grade 3, the far-right point is Census Division 9, Wyoming, or grade 8. This figure indicates that results are not driven by the experiences of a single state or geographic area. Consistent with Table 2.6, math performance gains tend to be larger for younger grades. All specifications include district, grade, and year fixed effects, as well as student racial/ethnic composition and segregation, student-teacher ratios, percent of students attending a charter school, child poverty rates and county unemployment rates. Bars denote 95 percent confidence intervals from robust standard errors clustered by district.

Figure B.6: Reading Performance: Drop Division, State, Grade

Notes: Figure plots coefficients and confidence intervals from the specifications in Table 2.5 for district-grade observations with a baseline FRP eligibility share below 57.9 percent (the baseline median among CEP districts) in which any school serving grade g participated in CEP by 2017, but dropping a single Census Division (blue), state (red), or grade (green). All omitted areas and grades are in ascending order (e.g.: the far-left point is Census Division 1, Alabama, or grade 3, the far-right point is Census Division 9, Wyoming, or grade 8. This figure indicates that results are not driven by the experiences of a single state or geographic area. Consistent with Table 2.6, math performance gains tend to be larger for younger grades. All specifications include district, grade, and year fixed effects, as well as student racial/ethnic composition and segregation, student-teacher ratios, percent of students attending a charter school, child poverty rates and county unemployment rates. Bars denote 95 percent confidence intervals from robust standard errors clustered by district.

Appendix C

Long-Term Gains from Longer School Days

C.1 Tables

Table C.1: Student Demographic Characteristics and JEC Exposure

	(1)	(2)	(3)	(4)
	Female	Indigenous	Mom has $<$ HS	Mom has $\geq$ BA
$\widehat{JEC}$	0.001	-0.001	-0.001	-0.001
	(0.003)	(0.002)	(0.003)	(0.006)
Observations	157698	157698	127853	48642
DV mean	0.517	0.104	0.440	0.212
E($\widehat{JEC}$)	2.020	2.020	1.756	2.086

Notes: Dependent variables are a series of indicators equal to one if a respondent reports a given demographic or socioeconomic characteristic at the time of the CASEN survey. All specifications include city of birth, survey year, and birth year-by-region fixed effects. $\widehat{JEC}$ defined as the expected years of full-day school attendance based on an individual's city and year of birth, calculated as described in Equation 3.1 from enrollment and JEC adoption data from the Ministry of Education; demographic characteristics from adults in our sample at the time of the 2006-2017 CASEN surveys. Sample limited to individuals born between 1979 and 1992 outside the Santiago metropolitan region who were 19-38 years old at the time of survey. Robust standard errors clustered by city of birth; all specifications weighted using regionally-representative weights. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table C.2: JEC Rollout Pace and Timing

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Year $\widehat{JEC} \geq 1$		Year $\widehat{JEC} \geq 0.01$		Year $\widehat{JEC} = 12$		Length of implementation	
Pct ages < 18	-0.715	8.820	0.354	7.080	1.796	10.926	1.442	3.846
	(13.171)	(11.023)	(9.244)	(10.619)	(13.238)	(11.260)	(10.173)	(9.700)
Pct ages > 65	-1.348	-17.774	-11.221	-6.583	0.219	-17.862	11.439	-11.279
	(17.765)	(15.914)	(14.061)	(16.802)	(17.847)	(16.023)	(14.855)	(16.073)
Pct in agriculture	0.753	0.995	1.160	0.101	0.615	0.833	-0.545	0.732
	(1.904)	(1.946)	(1.591)	(1.775)	(1.943)	(2.001)	(1.341)	(1.440)
Literacy rate	-0.126	14.959	6.673	7.167	1.014	15.892	-5.659	8.725
	(8.973)	(10.053)	(7.355)	(9.328)	(9.082)	(10.235)	(7.471)	(9.831)
Log pop	0.683	0.436	-0.680*	-0.731*	0.675	0.428	1.354***	1.159***
	(0.403)	(0.380)	(0.275)	(0.316)	(0.406)	(0.387)	(0.311)	(0.298)
Observations	131	131	131	131	131	131	131	131
FE	None	Region	None	Region	None	Region	None	Region
R2	0.065	0.483	0.131	0.269	0.067	0.469	0.323	0.576

Notes: Columns show relationships between baseline characteristics from the 1992 Census and the first year a birth cohort would be expected to have at least 1 year of full-day schooling (columns (1)-(2)); the first year a birth cohort would be expected to have any access to full-day schooling (at least 0.01 years, columns (3)-(4)); the first year a birth cohort would be expected to have any access to full-day schooling throughout its academic career (12 years, columns (5)-(6); the municipality-specific duration of the rollout period (year in columns (5-6) minus year in columns (3-4) in columns (7)-(8)). $\widehat{JEC}$ defined as the expected years of full-day school attendance based on an individual's city and year of birth, calculated as described in Equation 3.1 from enrollment and JEC adoption data from the Ministry of Education; municipality variables from the 1992 Census. Robust standard errors clustered by city of birth; all specifications weighted using regionally-representative weights. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table C.3: Robustness: Longer School Days and High School Graduation

	(1)	(2)	(3)	(4)
	Panel a: All			
$\widehat{JEC}$	0.020***	0.020***	0.015***	0.015***
	(0.002)	(0.002)	(0.002)	(0.002)
Observations	248535	248535	317260	317260
DV mean	0.794	0.794	0.812	0.812
E($\widehat{JEC}$)	2.925	2.925	2.845	2.845
	Panel b: Women			
$\widehat{JEC}$	0.023***	0.024***	0.017***	0.017***
	(0.002)	(0.002)	(0.003)	(0.003)
Observations	126929	126929	161733	161733
DV mean	0.808	0.808	0.825	0.825
E($\widehat{JEC}$)	2.902	2.902	2.823	2.823
	Panel c: Men			
$\widehat{JEC}$	0.017***	0.017***	0.013***	0.013***
	(0.003)	(0.003)	(0.003)	(0.003)
Observations	121606	121606	155527	155527
DV mean	0.779	0.779	0.798	0.798
E($\widehat{JEC}$)	2.949	2.949	2.868	2.868
Region X cohort FE		X	X	X
Cohort FE	X			
Includes Santiago			X	X
Baseline trends				X

Table C.3: (continued)

	(1)	(2)	(3)	(4)
	Panel d: Low SES			
$\widehat{JEC}$	0.023***	0.022***	0.021***	0.021***
	(0.003)	(0.003)	(0.003)	(0.003)
Observations	113172	113172	136569	136569
DV mean	0.701	0.701	0.715	0.715
E($\widehat{JEC}$)	2.757	2.757	2.645	2.645
	Panel e: High SES			
$\widehat{JEC}$	0.006***	0.008***	0.004*	0.004**
	(0.002)	(0.002)	(0.002)	(0.002)
Observations	79169	79169	109949	109949
DV mean	0.926	0.926	0.928	0.928
E($\widehat{JEC}$)	3.083	3.083	3.002	3.002
Region X cohort FE		X	X	X
Cohort FE	X			
Includes Santiago			X	X
Baseline trends				X

Notes: Dependent variable is an indicator equal to one if the respondent had completed high school at the time of the CASEN survey. All specifications include city of birth and survey year fixed effects. Control variables include current municipality of residence employment and poverty rates, gender, a quadratic in age, indigenous identity, and maternal education, as well as survey year linear trends in baseline poverty and employment rates by municipality of birth from the 1996 CASEN. $\widehat{JEC}$ defined as the expected years of full-day school attendance based on an individual's city and year of birth, calculated as described in Equation 3.1 from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2006-2017 CASEN surveys. Sample limited to individuals born between 1979 and 1992 who were 19-38 years old at the time of survey. Columns (1-3) omit trends in baseline employment and poverty rates; column (1) additionally replaces region-by-cohort fixed effects with cohort fixed effects. Columns (3-4) include respondents born in Santiago. Panel (b) and (c) limit the sample to women and men, and panels (d) and (e) limit the sample to individuals whose mothers had less than a high school education or at least a high school education, respectively. Individuals not reporting maternal educational attainment are excluded from panels (d-e). Robust standard errors clustered by city of birth; all specifications weighted using regionally-representative weights. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table C.4: Longer School Days and College Enrollment

	(1) All	(2) Women	(3) Men	(4) Low SES	(5) High SES
$\widehat{JEC}$	0.008***	0.006*	0.009***	0.010***	0.007
	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
Observations	172681	88972	83709	77796	52510
DV mean	0.450	0.464	0.435	0.305	0.675
Pct change	0.017	0.013	0.021	0.033	0.010
E($\widehat{JEC}$)	1.958	1.944	1.973	1.768	2.041

Notes: Dependent variable is an indicator equal to one if the respondent had attended at least some college at the time of the CASEN survey. All specifications include city of birth, survey year, and birth year-by-region fixed effects. Control variables include current municipality of residence employment and poverty rates, gender, a quadratic in age, indigenous identity, and maternal education, as well as survey year linear trends in baseline poverty and employment rates by municipality of birth from the 1996 CASEN. $\widehat{JEC}$ defined as the expected years of full-day school attendance based on an individual's city and year of birth, calculated as described in Equation 3.1 from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2006-2017 CASEN surveys. Sample limited to individuals born between 1979 and 1992 outside the Santiago metropolitan region who were 22-38 years old at the time of survey. Columns (2) and (3) limit the sample to women and men, and columns (4) and (5) limit the sample to individuals whose mothers had less than a high school education or at least a high school education, respectively. Individuals not reporting maternal educational attainment are excluded from columns (4) and (5). Robust standard errors clustered by city of birth; all specifications weighted using regionally-representative weights. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table C.5: Robustness:Longer School Days and College Graduation

	(1)	(2)	(3)	(4)
		Panel a: All		
$\widehat{JEC}$	0.012***	0.013***	0.007	0.008
	(0.002)	(0.003)	(0.005)	(0.005)
Observations	172681	172681	220786	220786
DV mean	0.182	0.182	0.203	0.203
E($\widehat{JEC}$)	1.958	1.958	1.898	1.898
		Panel b: Women		
$\widehat{JEC}$	0.007***	0.008***	0.004	0.005
	(0.003)	(0.003)	(0.006)	(0.006)
Observations	88972	88972	113695	113695
DV mean	0.199	0.199	0.217	0.217
E($\widehat{JEC}$)	1.944	1.944	1.890	1.890
		Panel c: Men		
$\widehat{JEC}$	0.016***	0.019***	0.011**	0.011*
	(0.003)	(0.003)	(0.005)	(0.006)
Observations	83709	83709	107091	107091
DV mean	0.164	0.164	0.188	0.188
E($\widehat{JEC}$)	1.973	1.973	1.906	1.906
Region X cohort FE		X	X	X
Cohort FE	X			
Includes Santiago			X	X
Baseline trends				X

Table C.5: (continued)

	(1)	(2)	(3)	(4)
	Panel d: Low SES			
$\widehat{JEC}$	0.003	0.004*	0.003	0.003
	(0.002)	(0.002)	(0.002)	(0.002)
Observations	77796	77796	93942	93942
DV mean	0.0992	0.0992	0.105	0.105
E($\widehat{JEC}$)	1.768	1.768	1.679	1.679
	Panel e: High SES			
$\widehat{JEC}$	0.015***	0.019***	0.003	0.002
	(0.005)	(0.005)	(0.011)	(0.012)
Observations	52510	52510	73447	73447
DV mean	0.309	0.309	0.327	0.327
E($\widehat{JEC}$)	2.041	2.041	1.986	1.986
Region X cohort FE		X	X	X
Cohort FE	X			
Includes Santiago			X	X
Baseline trends				X

Notes: Dependent variable is an indicator equal to one if the respondent had received a university degree at the time of the CASEN survey. All specifications include city of birth and survey year fixed effects. Control variables include current municipality of residence employment and poverty rates, gender, a quadratic in age, indigenous identity, and maternal education, as well as survey year linear trends in baseline poverty and employment rates by municipality of birth from the 1996 CASEN. $\widehat{JEC}$ defined as the expected years of full-day school attendance based on an individual's city and year of birth, calculated as described in Equation 3.1 from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2006-2017 CASEN surveys. Sample limited to individuals born between 1979 and 1992 outside the Santiago metropolitan region who were 23-38 years old at the time of survey. Columns (1-3) omit trends in baseline employment and poverty rates; column (1) additionally replaces region-by-cohort fixed effects with cohort fixed effects. Columns (3-4) include respondents born in Santiago. Panel (b) and (c) limit the sample to women and men, and panels (d) and (e) limit the sample to individuals whose mothers had less than a high school education or at least a high school education, respectively. Individuals not reporting maternal educational attainment are excluded from panels (d-e). Robust standard errors clustered by city of birth; all specifications weighted using regionally-representative weights. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table C.6: Robustness: Longer School Days and Employment in the Previous Month

	(1)	(2)	(3)	(4)
	Panel a: All			
$\widehat{JEC}$	0.010***	0.009***	0.006*	0.006*
	(0.003)	(0.003)	(0.003)	(0.003)
Observations	157696	157696	201983	201983
DV mean	0.655	0.655	0.675	0.675
E($\widehat{JEC}$)	2.021	2.021	1.947	1.947
	Panel b: Women			
$\widehat{JEC}$	0.012***	0.013***	0.008*	0.008*
	(0.004)	(0.004)	(0.005)	(0.005)
Observations	81210	81210	103958	103958
DV mean	0.542	0.542	0.576	0.576
E($\widehat{JEC}$)	2.008	2.008	1.945	1.945
	Panel c: Men			
$\widehat{JEC}$	0.008**	0.005*	0.003	0.003
	(0.003)	(0.003)	(0.003)	(0.003)
Observations	76486	76486	98025	98025
DV mean	0.776	0.776	0.780	0.780
E($\widehat{JEC}$)	2.034	2.034	1.949	1.949
Region X cohort FE		X	X	X
Cohort FE	X			
Includes Santiago			X	X
Baseline trends				X

Table C.6: (continued)

	(1)	(2)	(3)	(4)
	Panel d: Low SES			
$\widehat{JEC}$	0.013***	0.011***	0.006*	0.006
	(0.003)	(0.003)	(0.003)	(0.003)
Observations	70419	70419	85113	85113
DV mean	0.649	0.649	0.667	0.667
E($\widehat{JEC}$)	1.872	1.872	1.766	1.766
	Panel e: High SES			
$\widehat{JEC}$	-0.006	-0.008	-0.009**	-0.009**
	(0.006)	(0.005)	(0.005)	(0.005)
Observations	48641	48641	68129	68129
DV mean	0.651	0.651	0.676	0.676
E($\widehat{JEC}$)	2.086	2.086	2.020	2.020
Region X cohort FE		X	X	X
Cohort FE	X			
Includes Santiago			X	X
Baseline trends				X

Notes: Employment defined as having income at least 30,000 pesos (approximately $50) in the past month. All specifications include city of birth and survey year fixed effects. Control variables include current municipality of residence employment and poverty rates, gender, a quadratic in age, indigenous identity, household size, maternal education, marital status and number and presence of children, interacted with gender, as well as linear survey year trends in baseline poverty and employment rates by municipality of birth from the 1996 CASEN. $\widehat{JEC}$ defined as the expected years of full-day school attendance based on an individual's city and year of birth, calculated as described in Equation 3.1 from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2006-2017 CASEN surveys. Sample limited to individuals born between 1979 and 1992 who were 23-38 years old at the time of survey. Columns (1-3) omit trends in baseline employment and poverty rates; column (1) additionally replaces region-by-cohort fixed effects with cohort fixed effects. Columns (3-4) include respondents born in Santiago. Panel (b) and (c) limit the sample to women and men, and panels (d) and (e) limit the sample to individuals whose mothers had less than a high school education or at least a high school education, respectively. Individuals not reporting maternal educational attainment are excluded from panels (d-e). Robust standard errors clustered by city of birth; all specifications weighted using regionally-representative weights. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table C.7: Robustness: Longer School Days and Monthly Earnings

	(1)	(2)	(3)	(4)	(5)	(6)
	IHS(earn)	Earnings (level)	Log(earn)	Log(earn)	Log(earn)	Log(earn)
			Panel a: All			
$\widehat{JEC}$	0.141***	20888.202***	0.048***	0.047***	0.029**	0.029**
	(0.037)	(3426.251)	(0.010)	(0.009)	(0.014)	(0.014)
Observations	157696	157696	157696	157696	201983	201983
DV mean (level, 1000s pesos)	318.596	318.596	318.596	318.596	352.110	352.110
E($\widehat{JEC}$)	2.021	2.021	2.021	2.021	1.947	1.947
			Panel b: Women			
$\widehat{JEC}$	0.180***	15326.417***	0.044***	0.048***	0.030*	0.030*
	(0.052)	(3755.015)	(0.012)	(0.011)	(0.016)	(0.016)
Observations	81210	81210	81210	81210	103958	103958
DV mean (level, 1000s pesos)	233.708	233.708	233.708	233.708	267.879	267.879
E($\widehat{JEC}$)	2.008	2.008	2.008	2.008	1.945	1.945
			Panel c: Men			
$\widehat{JEC}$	0.108***	27722.169***	0.051***	0.046***	0.029**	0.029*
	(0.041)	(4501.885)	(0.011)	(0.010)	(0.014)	(0.015)
Observations	76486	76486	76486	76486	98025	98025
DV mean (level, 1000s pesos)	409.536	409.536	409.536	409.536	441.759	441.759
E($\widehat{JEC}$)	2.034	2.034	2.034	2.034	1.949	1.949
Region X cohort FE	X	X		X	X	X
Cohort FE			X			
Includes Santiago					X	X
Baseline trends	X	X				X

Table C.7: (continued)

	(1)	(2)	(3)	(4)	(5)	(6)
	IHS(earn)	Earnings (level)	Log(earn)	Log(earn)	Log(earn)	Log(earn)
Panel d: Low SES						
$\widehat{JEC}$	0.141***	12544.780***	0.044***	0.038***	0.023**	0.023**
	(0.045)	(2789.717)	(0.009)	(0.009)	(0.009)	(0.009)
Observations	70419	70419	70419	70419	85113	85113
DV mean (level, 1000s pesos)	246.378	246.378	246.378	246.378	260.551	260.551
E($\widehat{JEC}$)	1.872	1.872	1.872	1.872	1.766	1.766
Panel e: High SES						
$\widehat{JEC}$	-0.063	15757.256*	0.010	0.009	-0.014	-0.015
	(0.069)	(8789.232)	(0.020)	(0.018)	(0.023)	(0.024)
Observations	48641	48641	48641	48641	68129	68129
DV mean (level, 1000s pesos)	414.115	414.115	414.115	414.115	455.890	455.890
E($\widehat{JEC}$)	2.086	2.086	2.086	2.086	2.020	2.020
Region X cohort FE	X	X		X	X	X
Cohort FE			X			
Includes Santiago					X	X
Baseline trends	X	X				X

Notes: Column (1) transforms real earnings by the inverse hyperbolic sine; column (2) reports income in levels; and columns (3-6) report $log(earnings + 1)$. All specifications include city of birth and survey year fixed effects. Control variables include current municipality of residence employment and poverty rates, gender, a quadratic in age, indigenous identity, household size, maternal education, marital status and number and presence of children, interacted with gender, as well as linear survey year trends in baseline poverty and employment rates by municipality of birth from the 1996 CASEN. $\widehat{JEC}$ defined as the expected years of full-day school attendance based on an individual's city and year of birth, calculated as described in Equation 3.1 from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2006-2017 CASEN surveys. Sample limited to individuals born between 1979 and 1992 who were 23-38 years old at the time of survey. Columns (1-2) include region-by-cohort fixed effects and survey year trends in baseline municipal employment and poverty rates. Columns (3-5) omit trends in baseline employment and poverty rates; column (3) replaces region-by-cohort fixed effects with cohort fixed effects. Columns (5-6) include respondents born in the Santiago region. Panel (b) and (c) limit the sample to women and men, and panels (d) and (e) limit the sample to individuals whose mothers had less than a high school education or at least a high school education, respectively. Individuals not reporting maternal educational attainment are excluded from panels (d-e). Robust standard errors clustered by city of birth; all specifications weighted using regionally-representative weights. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table C.8: Robustness:Longer School Days and Log Monthly Earnings among Workers

	(1)	(2)	(3)	(4)
Panel a: All				
$\widehat{JEC}$	0.034***	0.036***	0.022**	0.021**
	(0.006)	(0.006)	(0.009)	(0.009)
Observations	101839	101839	132667	132667
DV mean (level, 1000s pesos)	486.183	486.183	521.441	521.441
E($\widehat{JEC}$)	1.904	1.904	1.829	1.829
Panel b: Women				
$\widehat{JEC}$	0.031***	0.032***	0.017*	0.016*
	(0.008)	(0.008)	(0.009)	(0.009)
Observations	42245	42245	56082	56082
DV mean (level, 1000s pesos)	430.530	430.530	464.728	464.728
E($\widehat{JEC}$)	1.910	1.910	1.846	1.846
Panel c: Men				
$\widehat{JEC}$	0.036***	0.037***	0.024**	0.024**
	(0.006)	(0.006)	(0.010)	(0.010)
Observations	59594	59594	76585	76585
DV mean (level, 1000s pesos)	527.866	527.866	565.991	565.991
E($\widehat{JEC}$)	1.900	1.900	1.816	1.816
Region X cohort FE		X	X	X
Cohort FE	X			
Includes Santiago			X	X
Baseline trends				X

Table C.8: (continued)

	(1)	(2)	(3)	(4)
Panel d: Low SES				
$\widehat{JEC}$	0.021***	0.019***	0.015***	0.015**
	(0.006)	(0.006)	(0.006)	(0.006)
Observations	44852	44852	54941	54941
DV mean (level, 1000s pesos)	379.556	379.556	390.543	390.543
E($\widehat{JEC}$)	1.824	1.824	1.707	1.707
Panel e: High SES				
$\widehat{JEC}$	0.041***	0.043***	0.017	0.017
	(0.012)	(0.013)	(0.017)	(0.017)
Observations	31351	31351	45011	45011
DV mean (level, 1000s pesos)	635.665	635.665	673.798	673.798
E($\widehat{JEC}$)	1.883	1.883	1.834	1.834
Region X cohort FE		X	X	X
Cohort FE	X			
Includes Santiago			X	X
Baseline trends				X

Notes: Dependent variable is defined as $log(earnings + 1)$ (in 2017 pesos). All specifications include city of birth and survey year fixed effects. Control variables include current municipality of residence employment and poverty rates, gender, a quadratic in age, indigenous identity, household size, maternal education, marital status and number and presence of children, interacted with gender, as well as linear survey year trends in baseline poverty and employment rates by municipality of birth from the 1996 CASEN. $\widehat{JEC}$ defined as the expected years of full-day school attendance based on an individual's city and year of birth, calculated as described in Equation 3.1 from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2006-2017 CASEN surveys. Sample limited to individuals born between 1979 and 1992 who were 23-38 years old at the time of survey and who report earned income of at least 30,000 pesos in the past month ($\approx$ \$50). Columns (1-3) omit trends in baseline employment and poverty rates; column (1) additionally replaces region-by-cohort fixed effects with cohort fixed effects. Columns (3-4) include respondents born in Santiago. Panel (b) and (c) limit the sample to women and men, and panels (d) and (e) limit the sample to individuals whose mothers had less than a high school education or at least a high school education, respectively. Individuals not reporting maternal educational attainment are excluded from panels (d-e). Robust standard errors clustered by city of birth; all specifications weighted using regionally-representative weights. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table C.9: Robustness: Longer School Days and Domestic Migration

	(1)	(2)	(3)	(4)	(5)	(6)
	Moved to Santiago	Log(Avg city residence income)	Moved	Moved	Moved	Moved
Panel a: All						
$\widehat{JEC}$	-0.001	0.015***	0.001	0.000	0.000	-0.001
	(0.002)	(0.005)	(0.003)	(0.003)	(0.003)	(0.003)
Observations	157696	157696	157696	157696	201983	201983
DV mean	0.110	0.0822	0.357	0.357	0.418	0.418
E($\widehat{JEC}$)	2.021	2.021	2.021	2.021	1.947	1.947
Panel b: Women						
$\widehat{JEC}$	0.000	0.017***	0.001	0.001	0.001	0.000
	(0.003)	(0.006)	(0.004)	(0.004)	(0.004)	(0.004)
Observations	81210	81210	81210	81210	103958	103958
DV mean	0.112	0.0811	0.365	0.365	0.428	0.428
E($\widehat{JEC}$)	2.008	2.008	2.008	2.008	1.945	1.945
Panel c: Men						
$\widehat{JEC}$	-0.003	0.013**	0.001	-0.000	-0.001	-0.002
	(0.003)	(0.006)	(0.003)	(0.003)	(0.005)	(0.004)
Observations	76486	76486	76486	76486	98025	98025
DV mean	0.108	0.0833	0.349	0.349	0.408	0.408
E($\widehat{JEC}$)	2.034	2.034	2.034	2.034	1.949	1.949
Region X cohort FE	X	X		X	X	X
Cohort FE			X			
Includes Santiago					X	X
Baseline trends	X	X				X

Table C.9: (continued)

	(1)	(2)	(3)	(4)	(5)	(6)
	Moved to Santiago	Log(Avg city residence income)	Moved	Moved	Moved	Moved
			Panel d: Low SES			
$\widehat{JEC}$	0.002	0.007*	0.003	0.001	-0.006	-0.007*
	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Observations	70419	70419	70419	70419	85113	85113
DV mean	0.0746	-0.0448	0.295	0.295	0.348	0.348
E($\widehat{JEC}$)	1.872	1.872	1.872	1.872	1.766	1.766
Panel e: High SES						
$\widehat{JEC}$	-0.002	0.025***	0.001	0.003	0.004	0.002
	(0.003)	(0.008)	(0.005)	(0.005)	(0.006)	(0.006)
Observations	48641	48641	48641	48641	68129	68129
DV mean	0.142	0.235	0.410	0.410	0.477	0.477
E($\widehat{JEC}$)	2.086	2.086	2.086	2.086	2.020	2.020
Region X cohort FE	X	X		X	X	X
Cohort FE			X			
Includes Santiago					X	X
Baseline trends	X	X				X

Notes: Dependent variable is an indicator = 1 if the respondent currently lives in a municipality in the Santiago region (col (1)); a standardized index of municipality per-capita income multiplied by whether the individual lives in a city other than his/her city of birth (col (2)); or an indicator = 1 whether the respondent currently lives in a city other than his/her city of birth (col (3-6)). All specifications include city of birth and survey year fixed effects. Control variables include current municipality of residence employment and poverty rates, gender, a quadratic in age, indigenous identity, household size, maternal education, marital status and number and presence of children, interacted with gender, as well as linear survey year trends in baseline poverty and employment rates by municipality of birth from the 1996 CASEN. $\widehat{JEC}$ defined as the expected years of full-day school attendance based on an individual's city and year of birth, calculated as described in Equation 3.1 from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2006-2017 CASEN surveys. Sample limited to individuals born 1979-1992 who were 23-38 years old at the time of survey. Col (1-2) include region-by-cohort fixed effects and survey year trends in baseline municipal employment and poverty rates. Col (3-5) omit trends in baseline employment and poverty rates; col (3) replaces region-by-cohort fixed effects with cohort fixed effects. Col (5-6) include respondents born in Santiago. Panel (b) and (c) limit the sample to women and men, and panels (d) and (e) limit the sample to individuals whose mothers had less than a high school education or at least a high school education, respectively. Individuals not reporting maternal educational attainment are excluded from panels (d-e). Robust standard errors clustered by city of birth; all specifications weighted using regionally-representative weights. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table C.10: Robustness: Longer School Days and Childbearing Patterns

	(1)	(2)	(3)	(4)	(5)
	Pr(teen mother)		Age at first birth		
Panel a: All Women					
$\widehat{JEC}$	-0.007***	0.198***	0.195***	0.107***	0.110***
	(0.002)	(0.042)	(0.047)	(0.039)	(0.038)
Observations	95874	28036	28036	67430	67430
DV mean	0.113	21.02	21.02	21.18	21.18
E($\widehat{JEC}$)	3.332	2.797	2.797	2.391	2.391
Panel b: Low SES					
$\widehat{JEC}$	-0.005*	0.180***	0.199***	0.062	0.062
	(0.003)	(0.050)	(0.058)	(0.046)	(0.046)
Observations	37940	13173	13173	25511	25511
DV mean	0.130	20.72	20.72	20.66	20.66
E($\widehat{JEC}$)	3.159	2.607	2.607	2.066	2.066
Panel c: High SES					
$\widehat{JEC}$	0.001	0.094	0.097	0.061	0.071
	(0.003)	(0.084)	(0.089)	(0.087)	(0.084)
Observations	33378	9964	9964	21220	21220
DV mean	0.0808	21.50	21.50	21.58	21.58
E($\widehat{JEC}$)	3.471	2.892	2.892	2.263	2.263
Region X cohort FE	X		X	X	X
Cohort FE		X			
Includes Santiago				X	X
Baseline trends	X				X

Notes: Dependent variable col (1) is an indicator = 1 if a woman gave birth before age 19 or (col (2)-(5)) is the age in years a woman gave birth to her first child. All specifications include city of birth and survey year fixed effects. Control variables include current municipality of residence employment and poverty rates, gender, a quadratic in age, indigenous identity, and maternal education, as well as survey year linear trends in baseline poverty and employment rates by municipality of birth from the 1996 CASEN. $\widehat{JEC}$ defined as the expected years of full-day school attendance based on an individual's city and year of birth, calculated as described in Equation 3.1 from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2006-2017 CASEN surveys. Sample limited to women born 1979-1992 who had given birth to at least one child at the time of the survey. Col (2-4) omit trends in baseline employment and poverty rates; column (2) replaces region-by-cohort fixed effects with cohort fixed effects. Col (4-5) include respondents born in Santiago. Panel (b) limits the sample to women whose mothers had less than a high school education; panel (c) limits the sample to women whose mothers had at least a high school education. Robust standard errors clustered by city of birth; all specifications weighted using regionally-representative weights. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table C.11: Robustness: Longer School Days and Occupational Upskilling

	(1)	(2)	(3)	(4)	(5)
	Log(avg occ wage)		Skilled occupation		
Panel a: All					
$\widehat{JEC}$	0.013***	0.010***	0.010***	0.007	0.006
	(0.003)	(0.003)	(0.003)	(0.005)	(0.005)
Observations	96211	101209	101209	131792	131792
DV mean	487.765	0.292	0.292	0.325	0.325
E($\widehat{JEC}$)	1.976	1.894	1.894	1.822	1.822
Panel b: Women					
$\widehat{JEC}$	0.012**	0.011*	0.010*	0.008	0.008
	(0.006)	(0.006)	(0.005)	(0.006)	(0.006)
Observations	40175	42220	42220	56060	56060
DV mean	473.531	0.379	0.379	0.391	0.391
E($\widehat{JEC}$)	1.975	1.899	1.899	1.839	1.839
Panel c: Men					
$\widehat{JEC}$	0.012***	0.009***	0.010***	0.006	0.005
	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
Observations	56036	58989	58989	75732	75732
DV mean	498.647	0.226	0.226	0.273	0.273
E($\widehat{JEC}$)	1.976	1.890	1.890	1.808	1.808
Region X cohort FE	X		X	X	X
Cohort FE		X			
Includes Santiago				X	X
Baseline trends	X				X

Table C.11: (continued)

	(1)	(2)	(3)	(4)	(5)
	Log(avg occ wage)		Skilled occupation		
Panel d: Low SES					
$\widehat{JEC}$	-0.005	0.001	-0.000	-0.001	-0.001
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Observations	41867	44828	44828	54882	54882
DV mean	417.984	0.179	0.179	0.195	0.195
E($\widehat{JEC}$)	1.909	1.812	1.812	1.703	1.703
Panel e: High SES					
$\widehat{JEC}$	0.029***	0.015**	0.017**	0.009	0.008
	(0.007)	(0.006)	(0.007)	(0.008)	(0.008)
Observations	29671	30829	30829	44296	44296
DV mean	597.607	0.456	0.456	0.484	0.484
E($\widehat{JEC}$)	1.949	1.874	1.874	1.823	1.823
Region X cohort FE	X		X	X	X
Cohort FE		X			
Includes Santiago				X	X
Baseline trends	X				X

Notes: Dependent variable is the average wage in a 4-digit occupation (col (1)) or an indicator = 1 if the respondent is employed in a managerial, technical, or professional occupation (col (2-5)). All specifications include city of birth and survey year fixed effects. Control variables include current municipality of residence employment and poverty rates, gender, a quadratic in age, indigenous identity, household size, maternal education, marital status and number and presence of children, interacted with gender, as well as linear survey year trends in baseline poverty and employment rates by municipality of birth from the 1996 CASEN. $\widehat{JEC}$ defined as the expected years of full-day school attendance based on an individual's city and year of birth, calculated as described in Equation 3.1 from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2006-2017 CASEN surveys. Sample limited to individuals born between 1979-1992 who were 23-38 years old at the time of survey. Military members and respondents without valid occupation codes are excluded. Col (1) includes region-by-cohort fixed effects and survey year trends in baseline municipal employment and poverty rates. Col (2-4) omit trends in baseline employment and poverty rates; col (2) replaces region-by-cohort fixed effects with cohort fixed effects. Col (4-5) include respondents born in the Santiago region. Panel (b) and (c) limit the sample to women and men, and panels (d) and (e) limit the sample to individuals whose mothers had less than a high school education or at least a high school education, respectively. Individuals not reporting maternal educational attainment are excluded from panels (d-e). Robust standard errors clustered by city of birth; all specifications weighted using regionally-representative weights. See text for details. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.10$.

Table C.12: Placebo Results: Effect of Longer School Days on Untreated Cohorts

	(1) HS grad	(2) College	(3) Log(earn)	(4) Skilled occupation	(5) Age at 1st birth
			Panel a: All		
$\widehat{JEC}$	0.006*	0.000	-0.003	-0.001	
	(0.003)	(0.001)	(0.006)	(0.002)	
Observations	166367	166367	166787	113608	
DV mean	0.497	0.112	371760.6	0.195	
Pct change	0.011	0.004		-0.004	
			Panel b: Women		
$\widehat{JEC}$	-0.000	-0.001	-0.008	0.003	-0.049
	(0.004)	(0.002)	(0.009)	(0.004)	(0.062)
Observations	88249	88249	88468	44673	51234
DV mean	0.501	0.108	219183.2	0.231	23.46
Pct change	0.000	-0.005		0.013	
			Panel c: Men		
$\widehat{JEC}$	0.012***	0.002	-0.001	-0.004	
	(0.004)	(0.002)	(0.007)	(0.003)	
Observations	78118	78118	78319	68935	
DV mean	0.492	0.118	550993.8	0.169	
Pct change	0.024	0.014		-0.022	

Table C.12: (continued)

	(1) HS grad	(2) College	(3) Log(earn)	(4) Skilled occupation	(5) Age at 1st birth
			Panel d: Low SES		
$\widehat{JEC}$	0.004	0.001	-0.008	0.002	-0.054
	(0.004)	(0.002)	(0.007)	(0.003)	(0.076)
Observations	91927	91927	92075	61747	27188
DV mean	0.438	0.0730	309771.4	0.145	23.09
Pct change	0.009	0.018		0.015	
			Panel e: High SES		
$\widehat{JEC}$	0.001	0.000	0.019	-0.018**	-0.371
	(0.006)	(0.006)	(0.022)	(0.008)	(0.230)
Observations	22217	22217	22263	16851	7872
DV mean	0.823	0.331	708507.9	0.440	24.67
Pct change	0.002	0.001		-0.040	

Notes: Dependent variables are defined as in Tables 3.3 through 3.9. All specifications include city of birth, survey year, and birth year-by-region fixed effects. Control variables include current municipality of residence employment and poverty rates, gender, a quadratic in age, indigenous status, maternal education, as well as survey year trends in baseline municipal employment and poverty rates. Columns (3)-(6) additionally include controls for household size, marital status and number and presence of children, interacted with gender. Placebo $\widehat{JEC}$ defined as the expected years of full-day school attendance based on an individual's city of birth and the access to JEC for individuals born 20 years later in the same municipality, calculated as described in Equation 3.1 from enrollment and JEC adoption data from the Ministry of Education; adult outcomes from the 2006-2017 CASEN surveys. Sample limited to individuals born between 1959 and 1972 outside the Santiago metropolitan region who were 43-58 years old at the time of survey. Panel (b) and (c) limit the sample to women and men, and panels (d) and (e) limit the sample to individuals whose mothers had less than a high school education or at least a high school education, respectively. Individuals not reporting maternal educational attainment are excluded from panels (d-e). Robust standard errors clustered by city of birth; all specifications weighted using regionally-representative weights. See text for details. *** $= p < 0.01$, ** $= p < 0.05$, * $= p < 0.10$.